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QUATERNARY TIME DIVISIBLE IN THREE PERIODS, THE LAFAYETTE, GLACIAL, AND RECENT.[1]

By Warren Upham.

According to definitions in text-books by Dana, Archibald Geikie and Etheridge, the Quaternary era began with the change from the mild Pliocene climate to that of the Glacial period, with its accumulation of the vast sheets of land ice in high latitudes, and has continued to the present time. We are living in the Quaternary era, as thus defined, and it must extend far into the future to be at all proportionate in length with the previous co-ordinate divisions of geologic time. Le Conte and Prestwich, however, consider the Quaternary division of time as completed at the dawn of civilization, with traditional and written history; and they assign recent geologic changes to a new era, named by Le Conte the Psychozoic, which is separated from the preceding principally on account of the supremacy of man. The former view seems preferable, because man is known to have been contemporaneous with the Ice age.

Quaternary time, therefore, is here assumed to include (1) the period of changed conditions causing the accumulation of

[1]Presented before Section E of the American Association for the Advancement of Science at the Brooklyn meeting, August 20, 1894; also partly contained in a paper read before the Geological Society of America, August 16, 1893, as published in its Bulletin, Vol. V, pp. 87–100, January, 1894.

the ice-sheets; (2) the Glacial period, when the glacial and modified drift were formed; and (3) the Postglacial, Recent, or Present period, extending from the departure of the ice-sheet until now. The first and second of these periods, which were comparatively long, constitute the Pleistocene division, while the third and very brief period is the Present or Psychozoic division, of the Quaternary era.

The Lafayette Period.

The broad lower part of the Mississippi Valley, from the southern boundary of the glacial drift to Louisiana, contains a very extensive unfossiliferous deposit of sand and gravel, designated formerly from its prevailing ferruginous color as the Orange sand, later called by McGee the Appomattox formation in its development on the costal plain of the Atlantic and Gulf States, but recently named the Lafayette formation, from Lafayette County in northern Mississippi, where it was earliest discriminated by Professor E. W. Hilgard in 1855 and 1856. This formation was spread across the valley plain 50 to 150 miles or more in width along an extent of 600 miles from the mouths of the Missouri and Ohio Rivers to the Gulf of Mexico, during the closing stage of the Tertiary era and the beginning of the Quaternary, to each of which it has been assigned. McGee,[2] Chamberlin[3] and Salisbury,[4] hold that it is probably referable to the Pliocene period; while Spencer,[5] Hilgard,[6] E. A. Smith[7] and others, as it seems to me preferably, have considered it as the earliest of our Pleistocene formations. Its northern continuation beneath the glacial drift is recognized by Salisbury[8] in western Illinois to a distance of a hun-

[2]Am. Journ. of Science, III, Vol. xxxv, February, April, May and June, 1888; Vol. xl, July, 1890. U. S. Geol. Survey, Twelfth An. Rep., for 1890–91, pp. 347–521, with 10 plates, and 45 figures in the text.

[3]Bulletin Geol. Soc. of America, Vol. i, 1890, pp. 469–480. Am. Jour. Sci., III, Vol. xli, May, 1891.

[4]Article last cited. Geol. Survey of Arkansas, An. Rep. for 1889 (published 1891). Vol. ii, "The Geology of Crowley's Ridge," pp. 224–248.

[5]Geol. Survey of Georgia, First An. Rept., for 1890–91, p. 62.

[6]Am. Jour. Sci , II, Vol. xlii, May, 1866; Vol. xlvii, Jan., 1869; Vol. xlviii, Nov. 1869; III, Vol. ii, Dec., 1871; Vol. xliii, May, 1892. Am. Geologist, Vol. viii, Aug., 1891, pp. 129–131.

[7]Am. Jour. Sci., III, Vol. xlvii, April, 1894.

[8]Bulletin Geol. Society of America, Vol. iii, 1892, pp. 183–186.

dred miles northward from the Missouri River and boundary of the drift, and gravels believed by him to be probably of the same formation occur in the Wisconsin and Minnesota driftless area, while northeastward he has observed the Lafayette gravels in the Ohio Valley in southern Indiana about 150 miles from the Mississippi. McGee states that the Lafayette beds attain their maximum thickness, which is 200 feet or more, in the region about the mouth of the Mississippi, and that they vary thence to a thin veneer, the thickness being proportional directly with the volume of neighboring rivers and inversely with the extension inland.

Previous to the maximum advance of the ice-sheet, the Mississippi River and all its large tributaries eroded deep and broad valleys through the Lafayette formation and underlying strata, cutting at New Orleans to a depth at least 760 feet below the present sea level. Along the central valley, from Cairo to the Gulf, this erosion averages probably 200 feet in depth upon a belt 500 miles long, with a width of 50 to 100 miles, excepting isolated plateau remnants of the Lafayette and older beds, of which the largest are Crowley's and Bloomfield ridges, in Arkansas and Missouri. The land during the valley erosion was certainly 760 feet higher than now, but this I think to be only a small fraction of its uplift. From the transportation of northern Archæan pebbles and cobbles of crystalline rocks to the Lafayette beds of the lower Mississippi and of Petite Anse Island, on the Gulf shore, in the direct line of the axis of the Mississippi Valley, Hilgard believes that during the deposition of these beds the valley had a greater descent and stronger currents of its river floods. He suggests that the increased altitude of the interior of the continent needed to give these formerly more powerful currents may have been 4000 to 5000 feet, being sufficient, probably, to bring the cold climate and ice accumulation of the Glacial period.

Marine submergence of the low coastal and Mississippi Valley areas occupied by the Lafayette formation is supposed by McGee and Spencer to have been requisite for the deposition of its sand and gravel beds, but they see that immediately

afterward the land was much higher than now, to permit the extensive and deep erosion of that time. A simpler view of the epeirogenic movements, closing the Tertiary era and inaugurating the Quaternary, seems to me to be found in ascribing these beds to deposition on land areas by flooded rivers descending from the Appalachian mountain region and from the Mississippi basin, spreading gravel, sand and loam over the coastal plain and along the great valley during the early part of a time of continental elevation. The land had lain during the long Tertiary periods at lower altitudes, and its surface was largely enveloped by residual clays and by alluvial sand and gravel. With the elevation of the continent, increased rainfall and snowfall and resulting river floods swept away these superficial materials from the higher lands and spread them on the coastal plain and along the Mississippi Valley, where the streams expanded over broad areas with shallow and slackened currents. As the elevation increased, however, the rivers would attain steeper slopes and finally erode much of the deposits which they had previously made. During the culmination of the uplift, which the writer believes to have been the chief cause of the Ice age, Chesapeake and Delaware Bays were excavated and erosion was in progress at a far more rapid rate than with the present low altitude of this region.

The Lafayette formation seems to me more closely related to the Glacial period and the conditions producing the ice-sheets than to the preceding very long Tertiary era, and for the same reasons which have been well stated by Hilgard and Spencer, namely, their dependence alike on the epeirogenic elevation.[9] With the Ice age we should unite this probably

[9]That epeirogenic movements of land elevation caused the accumulation of the Pleistocene ice-sheets, and conversely, that the end of the Glacial period was due to land depression, I have shown in an appendix of Wright's "Ice Age in North America," 1889, pp. 573–595; the Am. Geologist, Vol. vi, pp. 327–339, Dec., 1890; and the Am. Journal of Science, III, Vol. xli, pp. 33–52, Jan., 1891; and same, Vol. xlvi, pp. 114–121, Aug., 1893. This view, which may be called the epeirogenic theory of the causes of the Ice age, has been gradually thought out in America by Dana, LeConte, Hilgard, Wright and others, and in Scotland by Jamieson. Its earliest announcement was in 1855, by Dana in his Presidential Address before this Association (Proc. A. A. A. S., Vol. ix, for 1855, pp. 28, 29; Am. Jour. Sci., II, Vol. xxii, pp. 328, 329, Nov., 1856).

much longer preglacial time of gradual uplift of the continent, and the Postglacial or Recent period in which we live, to form together the three successive parts of the Quaternary era. How long the early part comprising the epeirogenic uplift, represented by the deposition and erosion of the Lafayette formation, may have been, we can only vaguely or perhaps approximately estimate. During the beginning of the uplift its effect would be probably to increase the transportation and deposition of gravel and sand by the rivers many times beyond their present action. The rate of average land erosion now prevailing throughout the drainage area of the Mississippi is supposed by McGee to be competent to supply in about 120,000 years a volume of river gravel, sand, and silt equal to the original Lafayette formation in the Mississippi Valley. With the greater altitude and increasing slopes of the land during the deposition of the Lafayette beds it may have required a third or a sixth of the time here mentioned, that is, some 40,000 or 20,000 years. As the elevation continued, however, rapid fluvial erosion of these deposits and of the underlying strata ensued, which was extended over so long and broad an area of the lower Mississippi Valley, and to such depth, that, even with the high continental elevation of 2000 to 3000 feet, known from submerged valleys off both the Atlantic and Pacific coasts, it must have required a long epoch. Perhaps it may be reasonably estimated twice as long as the time of the deposition, or somewhere between 40,000 and 80,000 years. The Lafayette period thus comprised two parts or epochs, the first characterized by deposition of the formation, the second by its extensive erosion and the culmination of the continental uplift.

The Glacial Period.

Comparison of the work of the glaciers and ice-sheets of the present time with those of Pleistocene time seems to me best accordant with a reference of all our glacial drift to a single continuous period of glaciation, which, though occupying probably 20,000 years or more, was yet brief as compared with the duration of most other recognized geologic periods or

epochs. The outflow of the upper part of the Pleistocene ice-sheets probably exceeded the currents of narrow alpine glaciers, but was less than the advance of broad and deep polar glaciers which end in the sea. For the journey of Pleistocene boulders 1000 miles in the ice-sheet, somewhat less than 3000 years would be required if the average of the glacial currents was five feet per day. The amount of the glacial erosion and of the drift, when compared with the erosion by the Muir glacier in Alaska, imply a short rather than a long duration of the Ice age. This conclusion is further affirmed by the continuance of the same species of the marine molluscan faunas from the beginning of the Glacial period to its end and to the present day.

The duration of the Ice age, if there was only one epoch of glaciation, with moderate temporary retreats and readvances of the ice-borders sufficient to allow stratified beds with the remains of animals and plants to be intercalated between accumulations of till, may have comprised only a few tens of thousands of years. On this point Prestwich has well written as follows: "For the reasons before given, I think it possible that the Glacial epoch—that is to say, the epoch of extreme cold—may not have lasted longer than from 15,000 to 25,000 years, and I would for the same reasons limit the time of . . . . the melting away of the ice-sheet to from 8000 to 10,000 years or less."[10]

Very gentle currents of broad river floods in the Missouri and Mississippi Valleys deposited the North American loess, attending the maximum extension of the ice-sheet and accompanying its departure up to the time of formation of the great marginal moraines. The loess thus testifies that previous to the farthest glacial advance the land sank to its present altitude, and probably somewhat lower on the area of the early drift, but not to the sea level. The vast weight of the continental glacier seems to have been the chief or only cause of this subsidence, as was first pointed out by Jamieson for the similar depression of the British Isles and Scandinavia at

[10]Quart. Jour. Geol. Soc., London, Vol. xliii, 1887, pp. 407, 408. Geology Vol. ii, 1888, p. 534.

the time of final melting of the European ice-sheet. The explanation of this continuance of the ice accumulation and advance after the depression of the land began and until the maxima, both of the land subsidence and ice extension, were attained, with a low altitude and even less descent of the lower Mississippi than now, has been well given by LeConte.[11] The subsidence was doubtless slow, even though probably many times faster than the preceding uplift. It may have occupied only 5000 years, being at a yearly rate of a half a foot to one foot; but possibly it was two or three times as long. While the slow sinking of the land was taking place, the accumulation of the ice by snowfall may have proceeded at a somewhat more rapid rate, so that the thickness of the ice-sheet and the altitude of its surface were increasing up to a maximum nearly coincident with that of the subsidence. Finally, however, the subsidence brought a warmer climate on the southern border of the ice, causing it to retreat, and giving to it in the region of the marginal moraines a mainly steeper frontal gradient and more vigorous currents than during its growth and culmination.

The time of general retreat of the ice-sheet in North America, with low altitude of the land and marine submergence of the coastal borders of northeastern New England, northward from Boston, and of the eastern provinces of Canada, with ingress of the sea along the valleys of the St. Lawrence and Ottawa Rivers and the basin of Lake Champlain, has been named by Dana the Champlain epoch. It was the final stage of the Glacial period, and was characterized by the rapid deposition of the glacial and modified drift, whose materials had been contained in the lower part of the ice-sheet.

The Postglacial, Recent, or Present Period.

Closely following the deposition of the modified drift as wide and deep flood-plains in the principal river valleys draining away from the departing ice, these beds were deeply eroded by the streams as soon as the ice-front had so far

[11]Bulletin Geol. Soc. of America, Vol. ii, 1891, pp. 329, 330. Elements of Geology, third edition, 1891, p. 589.

receded that the supplies of water and drift from its melting ceased. Much of the valley drift was soon removed by the river channelling, and its remnants, being left as terraces on the sides of the valleys, caused this first stage of the Postglacial period to be long ago named by Dana the Terrace epoch. In less vigorous action the streams have continued at the same work to the present day, so that this term may be extended also to comprise this whole period.

In various localities we are able to measure the present rate of erosion of gorges below waterfalls, and the length of the postglacial gorge divided by the rate of recession of the falls gives approximately the time since the Ice age. Such measurements of the gorge and falls of St. Anthony by Professor N. H. Winchell, show the length of the Postglacial or Recent period in Minnesota to have been about 8000 years; and from the surveys of Niagara Falls, Mr. G. K. Gilbert estimated it to have been 7000 years, more or less. From the rates of wave-cutting along the sides of Lake Michigan and the consequent accumulation of sand around the south end of the lake, Dr. E. Andrews believes that the land there became uncovered from its ice-sheet not more than 7,500 years ago. Professor G. Frederick Wright obtains a similar result from the rate of filling of kettle-holes among the gravel knolls and ridges called kames and eskers, and likewise from the erosion of valleys by streams tributary to Lake Erie; and Professor Ben. K. Emerson, from the rate of deposition of modified drift in the Connecticut Valley at Northampton, Mass., thinks that the time since the Glacial period cannot exceed 10,000 years. An equally small estimate is also indicated by the studies of Gilbert and Russell for the time since the last great rise of the Pleistocene lakes Bonneville and Lahontan, lying in Utah and Nevada, within the arid Great Basin of interior drainage, which are believed to have been contemporaneous with the great extension of ice-sheets upon the northern part of the North American continent.

Professor James Geikie maintains that the use of paleolithic implements had ceased, and that early man in Europe made neolithic (polished) implements, before the recession of the

ice-sheet from Scotland, Denmark and the Scandinavian peninsula; and Prestwich suggests that the dawn of civilization in Egypt, China and India may have been coeval with the glaciation of northwestern Europe. In Wales and Yorkshire the amount of denudation of limestone rocks on which drift boulders lie has been regarded by Mr. D. Mackintosh as proof that a period of not more than 6000 years has elapsed since the boulders were left in their positions. The vertical extent of this denudation, averaging about six inches, is nearly the same with that observed in the southwest part of the Province of Quebec by Sir William Logan and Dr. Robert Bell, where veins of quartz marked with glacial striæ stand out to various heights not exceeding one foot above the weathered surface of the enclosing limestone.

From this wide range of concurrent but independent testimonies, we may accept it as practically demonstrated that the ice-sheets disappeared only 6000 to 10,000 years ago. Within this period are to be comprised the successive stages of man's development of the arts, from the time when his best implements were made of polished stone through the ages of bronze, iron, and finally steel, to the present time when steel, steam and electricity seem to bring all nations into close alliance.

Estimated Duration of the Quaternary Era.

Arranged in chronologic order, we have derived for the three parts of the Quaternary era, as here defined, the following estimates of their duration: the Lafayette period or time of preglacial epeirogenic elevation, with the deposition and erosion of the Lafayette beds, some 60,000 to 120,000 years; the Glacial period, regarded as continuous, without interglacial epochs, attending the culmination of the uplift, but terminating after the subsidence of the glaciated region, 20,000 to 30,000 years; and the Postglacial or Recent period, extending to the present time, 6000 to 10,000 years. In total, the Quaternary era in North America, therefore, has comprised probably about 100,000 or 150,000 years, its latest third or fourth part being the Ice age and subsequent time. The Tertiary era appears by the changes of its molluscan faunas to have been

vastly longer, having comprised, perhaps, between two and four million years, of which the Pliocene period would be a sixth or eighth part, thus exceeding the whole of the ensuing era of great epeirogenic movements and resulting glaciation.

Divisions of Quaternary Time.

The following table of the several divisions, periods and epochs of Quaternary time, as reviewed in this paper, is arranged in the descending stratigraphic order of their geologic formations.

Psychozoic division	Recent period	Recent or Present epoch.
		Terrace epoch.
Pleistocene division	Glacial period	Champlain epoch.
		Glacial epoch.
	Lafayette period	Epoch of great elevation and erosion.
		Lafayette epoch.

THE HOMOLOGIES OF THE UREDINEAE (THE RUSTS).

BY CHARLES E. BESSEY.

The place of the parasitic plants constituting the Order *Uredineae* (The Rusts), in a natural system of classification, has long been in doubt, botanists not being fully agreed as to the homologies existing between these and other fungi. In a study of this group, extending over many years, I have been led to a view of the homologies between these plants and the Ascomyceteae and Basidiomyceteae, somewhat at variance with the theories of most recent writers; and it is probable that the time has come for a more definite statement of this view than has yet been given.

GENERAL STRUCTURE.

The *Uredineae* are parasitic within the tissues of higher plants, for the most part Anthophyta. They consist of septated branching threads which vegetate for some time within the host, and eventually produce spores (*conidia*) in chains, by abstriction. These spores develop upon numerous, crowded, parallel, terminal branches, within the tissues of the host, at length bursting through the epidermis. The outer conidial branches are modified into a "peridium," which surrounds the erumpent spore-mass like a tiny cup, whence the common name, "Cluster-cup," in allusion also to the fact that the spore-cups usually appear upon the leaf in clusters. For a long time these cluster-cups were supposed to have no connection with the rusts, and they accordingly were described under the generic names *Aecidium* and *Roestelia.* The first of these names is preserved in the term "aecidiospore," by which the spores are often designated. (Figs. I and II of Plate XXXII.)

Somewhat later, spores of another kind are produced singly upon the ends of other branches in the tissues of the host. These, while occurring in clusters, are by no means as closely

and regularly crowded as the aecidiospores, so that when they burst through the epidermis of the host they constitute elongated or irregular shaped spore-dots (*sori*) instead of definitely outlined cups. Here again, the spores of this kind were regarded by the earlier botanists as belonging to a distinct genus, *Uredo :* hence we commonly still speak of them as *uredospores.* They are also known as "stylospores," in allusion to the fact that they are stalked. (Figs. III and IV of Plate XXXII.)

Still later, a third kind of spore is produced, often in the uredosori, which bear some resemblance to the uredospores in being stalked, and in some cases, one-celled (*Uromyces, Melampsora*), but differing often in being two or more celled, and usually having a thicker wall. These are the last to develop upon the mycelium within the host, and when they have ripened, usually the parasite dies. Since these spores appear to complete the development of the parasite, they have long been known as teleutospores (τελευτή, "completion.") They germinate (in many species after a period of rest through the winter months) by the production of a short, several-jointed filament (the *promycelium*), from each cell of which short lateral branches develop, upon whose summits single minute spores (sporidia) are formed by abstriction. When these sporidia germinate upon the proper host they form parasitic threads which penetrate its tissues and give rise to the aecidia described above, thus completing the cycle of life. (Figs. V to XIII of Plate XXXII.)

The life history here sketched may be taken as typical, but it is subject to several modifications, e. g., (*a*) the omission of the aecidial stage; (*b*) the omission of the uredo stage; (*c*) the omission of both the aecidial and the uredo stages. Moreover, in many species the aecidial stage occurs upon a different host from that which supports the uredo and teleutospore stages, this condition being known as heteroecism, a familiar example of which may be seen in one of the common rusts of wheat (*Puccinia graminis*), where the aecidiospores develop on the leaves of the Barberry (*Berberis vulgaris*), the uredospores and teleutospores alone occurring in the leaves and stems of

the wheat. In many heteroecismal species it has hitherto been found impossible to determine the aecidium belonging to it, and for many aecidia occurring upon common plants, the uredo and teleutospore stages are not known. The difficulties surrounding this problem are so great as to discourage the attempt to solve them.

Homology of Parts.

Having now a general idea of the structure of the *Uredineae*, we come to the important question of the homology of their parts. Here, again, we are beset with difficulties. No sexual organs have yet been discovered, and there has been very much structural degeneration of the whole plant.

In their general structure the Uredineae show clearly that their relationship is with the *Ascomyceteae* or *Basidiomyceteae* rather than with the *Phycomyceteae*, and upon this point there has been little disagreement among recent botanists. Some authors regard the aecidium as a kind of degenerated apothecium, in which each conidial chain is a modified ascus. In this view, the aecidium is the result of an obsolete or obsolescent sexual act, as in the *Discomyceteae*, and the uredospores and teleutospores are considered to be conidial structures. Accordingly, those who hold this view quite consistently set off the *Uredineae* in a class bearing the name *Aecidiomycetes*. By far the greater number of botanists, however, now regard the teleutospores as basidia, homologous with the basidia of the *Hymenomyceteae* and *Gasteromyceteae*, and they therefore place the *Uredineae* in the class *Basidiomyceteae*. In this view, the sporidia which develop upon the germination of the teleutospore are basidiospores, homologous with those of mushrooms and puff balls, and the uredospores and aecidiospores are forms of conidia. It is needless in this paper to set forth these views at length, since they may be found in almost any common text-book of botany.

Briefly stated, the view which I wish to present is that the "teleutospore," so-called, is a tightly fitting ascus, containing one or more large spores; the teleutosorus is a reduced apothecium; the aecidiospores are the normal conidia; and the

uredospores secondary or accessory conidia (stylospores). In many cases the ascus-wall is readily separable from the contained spore or spores; but for the most part, the ascus-wall is so closely adherent as not to be distinguished from the spore-wall without treatment by potassic hydrate or other reagents.

In one genus, *Uropyxis*, the ascus is much larger than the double spore it contains, and may be observed very easily without special preparation. (Fig. VIII of Plate XXXII.) In *Gymnosporangium* in fresh material an ascus cavity considerably larger than the double spore can be seen in carefully made preparations. Young "teleutospores" of *Phragmidium*, in which the spores have not yet attained full size, show the ascus-wall very clearly, (Fig. IX of Plate XXXII), although in mature specimens by the enlargement of the spores it can be seen with difficulty, if at all. By careful examination, one may make out the ascus-wall in a good many cases where otherwise it might be overlooked. I have little difficulty in distinguishing it in some species of *Uromyces* (where the ascus contains but one spore) and *Puccinia* (where the ascus contains one double spore, or more accurately speaking, two spores), especially after the application of strong potassic hydrate.

The Question of Relationship.

The view here set forth, that the so-called "teleutospore" is an ascus with its contained spore or spores, involves the supposition that the *Uredineae* have suffered much structural degeneration. When we consider the fact that they are, as we may say, *intensely* parasitic, there is no improbability that we are dealing here with a greatly reduced plant structure. One has but to contrast a Dodder with a Morning Glory, or a Broom-Rape (*Aphyllon*) with a Figwort (*Scrophulariaceae*) in order to realize what great changes are produced by a parasitic habit. It has long been well known in biology that the greater the parasitism of an organism the greater is its degeneration. Some plants take but little from their hosts, and still maintain their roots, stems and leaves with so little change

that it is scarcely perceptible. It is said that some of the Gerardias are parasitic, and yet who can perceive in the countenance of any of our species any evidence of this particular vegetable sin? The closely related painted cups (*Castilleia*), however, give evidence in their appearance that their habits are not what they should be. It is even more so with *Comandra*, while the Mistletoe bears the marks of degradation upon every organ. It is not otherwise with the Carpophytes. When some ancestral seaweeds became saprophytic and parasitic, that structural degeneration of parts began which gave us the many kinds of fungi. No one may now trace with certainty the genetic line of the fungi, but that they originated from holophytic ancestors cannot be doubted; nor can there be reasonable doubt that they have become structurally more and more modified the further they have departed from holophytic habits. The holophyte requires masses of chlorophyll-bearing cells, or as we commonly express it, its vegetative organs must be well developed, but the hysterophyte has no use for such tissue, and consequently, its vegetative organs are undeveloped. The more perfectly the parasite adapts itself to its host the greater may be its departure from the structure of its vegetative organs which its holophytic ancestors developed. In like manner, the more perfectly the parasite merges itself into its host, and in a sense becomes a part of it, the more may it use the host tissues for protection and support, and the less is it necessary for it to develop protective tissues of its own. Thus we have in the fungi not only a degeneration of the vegetative tissues, but the reproductive organs have likewise undergone much degenerative modification.

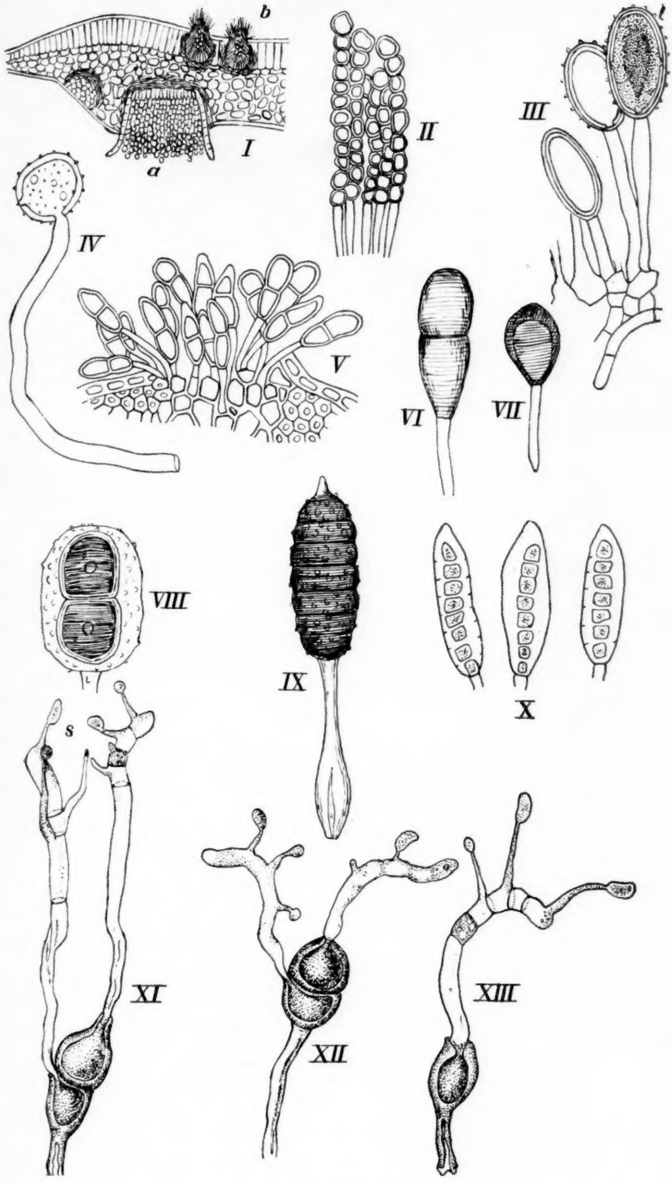
We here regard the *Uredineae* as degenerated Cup-Fungi (*Discomyceteae*), with their cups (apothecia) obsolescent, and constituting the vaguely defined teleutosori. As suggested above, there is here no need of that abundant accessory tissue which in the Cup-Fungi forms a protective envelope (exciple) around the hymenial mass, since the asci ("teleutospores") develop beneath the protecting epidermis of the host. The host-tissues in the case of the *Uredineae*, act the part of the exciple in the normal cup-fungi. The apothecia of the cup-

fungi are therefore homologous with the "sori" of the teleutospore stage of the *Uredineae.* Instead of the large eight spored asci, which are so common in the *Discomyceteae,* we find in the *Uredineae* that they are much reduced, both in size and the number of spores which they contain, there being rarely more than one or two. And here we may propose, in the light of the view here adopted, that the term "teleutospore," while a misnomer as usually applied, be retained with a restricted application to the spore or spores within the ascus. Thus we may say that the ascus of *Uromyces* contains but one teleutospore, while in *Phragmidium* it contains several. If necessary (which I doubt) to distinguish these reduced asci from normal ones, we may employ the convenient term *teleutoasci.* We may thus have *teleutosorus, teleutoascus* and *teleutospore.*

Place in the System of Plants.

It remains to say a few words as to the place in the system of plants to be assigned to the *Uredineae* in accordance with these views. From what has been said, it follows that they are to be regarded as *Ascomyceteae,* instead of *Basidiomyceteae,* as so many recent botanists assert. Further, it is held that they are degraded and much modified forms standing at or near the end of a long genetic line, and not primitive or ancestral forms from which higher and more complex ones have sprung. The cup-fungi have not been derived from the *Uredineae,* but rather we may say that, in all probability, the latter have been derived by degeneration from the former. We must, therefore, assign the *Uredineae* to a place in the *Ascomyceteae, after* the *Dicomyceteae.* All may well agree to assign the *Perisporiaceae* to the first (or lowest) place in the class on account of their slight modification from the type of the holophytic Carpophytes. From this primitive group we pass easily along three somewhat divergent genetic lines, viz.: the *Tuberoideae, Pyrenomyceteae,* and *Discomyceteae,* and from the latter have sprung the *Uredineae.* The arrangement will then be as follows:

PLATE XXXII.



Uredineæ.

CLASS ASCOMYCETEAE.

Order *Perisporiaceae,*
Order *Tuberoideae,*
Order *Pyrenomyceteae,*
Order *Discomyceteae,*
Order *Uredineae,*
Order *Ustilagineae.*

CLASS BASIDIOMYCETEAE.

Order *Gasteromyceteae,*
Order *Hymenomyceteae.*

Of the relationship of the *Uredineae* to the *Ustilagineae* I need say no more at the present time than that the latter are here regarded as still further degradations of the *Discomyceteae;* nor is this the place in which to take up a discussion of the homologies between the *Ascomyceteae* and the *Basidiomyceteae.* Upon the latter point it is sufficient to say that the ascus and the basidium are regarded as morphologically equivalent, the ascus subdiving its protoplasmic contents into spores by an internal division (forming ascospores) while the basidium accomplishes the same thing by the growth of protrusions ("sterigmata") into whose enlarged ends the protoplasm passes, after which they separate as spores (basidiospores).

EXPLANATION OF PLATE XXXII.

I. Cross section of a Barberry leaf; *a*, a cup of aecidiospores; *b*, spermogones of *Puccinia graminis*, after Luerssen × 40.

II. Rows of aecidiospores (conidia) of *P. graminis* upon their conidiophores, after De Bary × 150.

III. Uredospores of *P. graminis*, the shaded one ripe, after De Bary, × 390.

IV. Germinating uredospore of *P. straminis*, after De Bary, × 390.

V. Cross section of a teleutosorus of *P. graminis*, after De Bary, × 200.

VI. Teleutoascus of *P. graminis*, external view, after Ludwig, × 450.

VII. Teleutoascus of *Uromyces fabae*, optical section, after Ludwig, × 450.

VIII. Teleutoascus of *Uropyxis amorphæ*, optical section, after Ludwig, × 450.

IX. Teleutoascus of *Phragmidium subcorticium*, external view, after Ludwig, × 450.

X. Immature teleutoasci of *Phragmidium subcorticium*, after Bessey, × 400.

XI. Germinating teleutospores (still within the ascus) of *Puccinia graminis; s.* sporidia, after Tulasne, × 400.

XII. Germinating teleutospores (still within the ascus) of *Puccinia moliniae*, after Tulasne, × 400.

XIII. Germinating teleutospore (within its ascus) of *Uromyces appendiculatus*, after Tulasne, × 400.

ON THE EVOLUTION OF THE ART OF WORKING IN STONE. A PRELIMINARY PAPER BY J. D. McGUIRE.

A REPLY BY CHARLES H. READ.

In the *American Anthropologist* for July, 1893, appeared an essay with the above title. The writer, with whom I am personally unacquainted, was good enough to send me a separate copy of it. I read it with some interest, for the efforts of an earnest worker, who attempts, by novel methods, to solve a difficult problem, cannot fail to be of interest to any one who has given attention to the problem itself.

The question of palæolithic man in America has, moreover, given rise to such fierce discussion that it seemed necessary to point out the danger that lies in the use of improper or irrelevant evidence. Such methods can only serve to mislead enquirers and to delay the solution of the puzzle. The paper now in question is so persistent in its pursuit of will-o'-the-wisps that a better text could scarcely be found.

The problem Mr. McGuire has set himself to resolve, stripped of all redundant matter, is this: whether the so-called palæolithic remains of Europe are necessarily older than the so-called neolithic? Incidentally he implies that "from a purely archæological standpoint, the paleoliths of Europe and the similar American implements are in all particulars, identical, and are productions of man existing under like conditions." What he understands by an archæological standpoint we shall see later, but first I would deal with the main contention.

Noscitur a socii is an axiom of archæology. When an object is found in the earth, and is dumb as to its own history, we naturally and justly turn to its companions to help us. This is good so far as it goes, and in an isolated case we may go wrong. But when we multiply the single case with fifty or a hundred, finding in all the same association of objects, and the circumstances attested by persons of known observation and probity, what before was probability is turned into as

great certainty as humanity can attain over the past. This, in a few words, is the foundation upon which paleolithic man in Europe now stands. This foundation might be broadened by much geological addition, but the argument would be none the more forcible. To put it more directly, certain flint implements are found in a stratum of a known age, so that this particular stratum comes to be recognized by all observers as their habitat. They are found elsewhere, truly, but when so found they usually bear indications of the vicissitudes they have undergone since leaving their home. Such flint implements, further, are found associated with the remains of animals which are universally admitted to belong to a given geological epoch. Here again they are so associated with such persistency, noted by such widely separated and independent observers, that the possibility of universal error is as wildly improbable as that of universal conspiracy. Such being the class of evidence upon which the antiquity of palæolithic man is founded, it is obvious that any attack, to be effectual, must be made on the premises. If it could be shown either that the palæolithic implements were not found in their undisturbed bed, or that the animal remains near them had no connection with them, then any conclusions based upon such association would necessarily fall to the ground.

Mr. McGuire takes, however, an entirely different stand. His theories are based upon his own experience as an amateur maker of stone implements, and his experiments have led him to the belief that it is *far easier* to make a polished stone implement than a chipped one, and that *therefore* polished *flint* implements are at least as old as those that are only chipped and not polished!

Has Mr. McGuire ever seen a specimen of Kafir or Polynesian carpentry? In the British Museum is a Kafir copy of a common European chair, made in the usual fashion as to shape, with slender spars for a back, a solid seat and spidery legs. This is cut from one solid block of wood, surely a far more difficult task than to make the chair by joining in the usual manner. Applying Mr. McGuire's argument to this case, and it does not seem an unfair application, for both the

Kafir and the Polynesian cuts everything from the solid, where does it land us? Are we to think that they began with joining, without doubt the easier method, and finally came to the more difficult, the cutting from the solid? Surely not; the natural explanation is the best, simply that the easier method of work did not occur to them.

From another point of view Mr. McGuire's experience is somewhat at variance with that of others. Palæolithic implements in Europe, and I would prefer to speak of Europe only at present, are made of very few materials, chiefly flint and quartzite. Mr. McGuire knows and admits this fact, but seems to assert that it is easier to form an implement by battering than by chipping. If the implement is to be of flint, I greatly doubt it, but if of certain stones of difficult or uncertain fracture, it may well be the case.[1] It seems inconceivable that such a statement could be calmly made, seeing how entirely contrary it is not only to the experience of all who have tried the experiment, with the single exception of Mr. McGuire, but also in direct opposition to all the evidence on the subject. Can Mr. McGuire point out a single instance of a polished implement being found on an admitted palæolithic site? He gives no such instance, and as it would form the strongest point in his whole argument if he could quote one, we may presume that he does not. That being so, surely it is fighting the air to bring a long array of his own experiments to prove that palæolithic man *ought* to have found out what he considers the easiest way of making his tools.

It may be well to make the point at issue quite clear by stating that there is no question of the polishing or grinding of an implement caused *by use.* Such an instance, probably more than one, of the chipped edge of a tool of palæolithic age being worn or ground by applying it to its destined work, has occurred. But it has never been urged that the effect thus produced was part of the original design.

Before leaving this branch of the enquiry I would fain quote Mr. McGuire's peroration. He says that palaeolithic man

[1] I say "seems to assert," for though the point at issue is the making of palaeolithic implements, yet Mr. McGuire uses the indefinite term "stone" when he should say "flint."

"had knives with which he could cut various articles and needles with which to sew; he knew the art of making and burning pottery; could and did make fire; he drilled holes of large and small size in bones, antlers, shells and fossils, and was familiar with the art of engraving at a period contemporaneous with the Mousterian implement and a quaternary fauna. With such evidence can it be argued that man was ignorant of a knowledge of the process by which stone was battered and ground in to shape and yet familiar with the more complicated art of chipping?"

On the other side I would put the man of the eighteenth century. He was familiar with the learning of two thousand years preceding his own time; he knew and practised the art of printing; he was an accomplished chemist and astronomer; he was an admirable artist in painting, sculpture and music; was a student of the forces of nature; traversed the whole world for the improvement of his mind or the bettering of his fortunes; he was expert in the beautifying of his every day surroundings of furniture and the accessories of a luxurious home. With such evidences should it not be argued with far greater force that he must have known that under the lid of his boiling tea-kettle, a utensil of daily use, lay a force that would carry him over land or sea five times more swiftly than the swiftest horse? Yet it is remarkable that he never thought of the application of the power of steam.

One word about the "purely archæological standpoint." This seems, in Mr. McGuire's view, to resolve itself into "the character and size of the chips detached appearing identical as do the so-called implements when laid one beside the other;" for, on the same page, he says, "Taking the type of the implement as a criterion of antiquity, America, Europe and Asia stand on the same footing." This, however, is the most dangerous criterion that could be taken. Even in Europe where the material used and the character of the sites are nearly alike, the type of implement alone is by no means a certain indication of age. I have seen hundreds of undoubted neolithic implements of far ruder work than an ordinary implement from the drift. And there is every reason why it should

be so. The material used is the same, and we have no ground for supposing that the process of manufacture was different. When, however, the types of one Continent are used as a criterion, by superficial resemblance alone, for determining the date of similar implements from another and distant Continent, the conclusions arrived at can obviously be of no value whatever.

I have long thought that a prominence totally undeserved has been given to the rule of thumb distribution that "chipped — polished = palaeolithic, and chipped + polished = neolithic." Its only virtue is its convenience and that it is easy to remember. But to exalt it to the dignity of a determinative factor is, I think, a great mistake, and I feel sure that many ardent collectors of stone implements cling to this accidental distinction as their sheet-anchor for data. The fact that palæolithic man overlooked the polishing of his implements is a mere accident, a subsidiary and incidental peculiarity, and possesses no right whatever to the importance it has attained. It has not the least value in determining whether an implement is of one or the other period. The converse of the proposition does not, of course, hold good in our present state of knowledge. If a polished implement of flint be found, it can safely be declared non-palæolithic, for the reason that up to now no implement with a designedly ground surface has been found on a palæolithic site. It would be of the greatest service in this particular if some fortunate searcher could light upon a hoard of polished palæolithic flint tools. Then it is possible that the true determination of palæolithic as opposed to neolithic would obtain proper recognition; that it does not rest upon the slender evidence of "chipping only," but upon a far more solid foundation, to wit, the evidence of the bed in which it lies.

To the observer in Europe the whole question of what is known as palæolithic man in America seems to be in a chaotic state. There appear to be many reasons for this. One principal one is, without doubt, the unfortunate reliance upon a particular type of implement as a distinguishing character of palæolithic deposits. Granted that such a type has a deter-

minative value in Europe, by what process of reasoning can it be argued that man, living thousands of miles away, has produced the same peculiar variety, simply because he lived with a similar group of extinct animals? Another reason, perhaps equally potent, is that only a very limited number of the students of early man in America have made any lengthened study on the spot of the conditions under which these remains are found in Europe. If the conditions are to be similar in America, then this would appear to be a necessity. If they are unlike, as is very possible, yet there must be sundry points of resemblance, and it is surely of value to proceed to the study of the unknown by familiarizing the mind with the date of a known and accepted condition. To sum up in a few words—let intelligent observers, trained to use their eyes, knowing what constitutes evidence, and capable of recording it, let such men work over the possible sites of the American Continent, and the result of their labors will, without any doubt, be of the greatest value to science, whether palæolithic man be found or not. But it is of the first importance that the explorers be trained men. The investigations of men without the necessary knowledge not only causes the results to be of little present value, but their work destroys the very evidence upon which alone true knowledge can be founded.

ZOOLOGY IN THE HIGH SCHOOL.[1]

By Clarence M. Weed.

I do not see how the program recommended by the Natural History Conference of the Committee of Ten[2] can escape the charge of being inadequate and one-sided. According to it, eight years of study of at least two periods each week are to be devoted to plants before the high school is reached. This study includes not only the various parts and functions of the higher plants, their classification and life-histories, but the lower plants as well. Then in the high school five exercises a week for one school year are to be devoted to what can be considered only as a systematic review of knowledge already acquired. In all the twelve years of school life no provision is made for the study of animals, except a brief term of physiology, unless the advice of the conference is ignored and zoology is substituted for botany in the high school course. Truly it would appear that the much abused term—natural history—is to be restricted once more and become a synonym of botany. That the Conference did not intend to restrict the nature study of the lower schools to plants is abundantly shown by their answers to the questions submitted by the Committee of Ten, in which they distinctly recommend the study of both plants and animals for these grades.

The Conference "agreed that the year of study in natural history, recommended as a minimum for the high school, should be a consecutive year of daily recitations or laboratory work, and it is better to have the year's work devoted to one subject, either botany or zoology, than to have it divided between the two." Two years have passed since this opinion was promulgated, and while it may have represented the best educational ideas concerning the study of biology then, there

[1] From a paper read before a High School Teachers' Institute, Concord, N. H., Sept. 21, 1894.

[2] Rept. of Committe on Secondary School Studies, pp. 138–158., U. S. Bereau of Education.

is abundant evidence to show that it does not to-day. For there are many indications that biological teachers are accepting and adopting the dictum long since enunciated by Huxley that "the study of living bodies is really one discipline, which is divided into zoology and botany simply as a matter of convenience." Nothing shows this more clearly than the general adoption of such books as Huxley & Martin's Course of Elementary Instruction in Practical Biology, Parker's Lessons in Elementary Biology, Dodge's Introduction to Elementary Practical Biology, and Boyer's Laboratory Manual in Elementary Biology. These books are designed for use in the high schools and colleges, and unquestionably represent the consensus of opinion among the most successful biological teachers. They show that the study of living things can easily be carried on in a consecutive course in which the student may obtain a basis of sound biological knowledge concerning the organisms on both sides of the imaginary fence which separates the plant and animal world. I doubt if any fair-minded zoologist would think of insisting on confining the biological training of high school students to animals, for it would be a one-sided and inadequate training introducing the pupil to one phase of nature when he is entitled to an introduction to both. No more should the botanist claim an exclusive privilege in this respect.

The reasons given by the Conference report for choosing botany instead of zoology are three, viz.: (1) "Because the materials for the study of that subject are probably more easily obtained than those for the study of zoology; (2) Because the study of plants is more attractive to the average pupil; and (3) Because, in the study of animals, many prejudices or aversions have to be overcome." Obviously, these last two causes should be considered as one, the explanation of the greater attractiveness of plants must largely be found in the prejudices and aversions to animals. My own experience in teaching both subjects leads to the opinion that there is little weight to be given the argument on either side: some students prefer one subject and some the other, but the greatest enthusiasm is always aroused by the study of animals like Vorticella, whose

life processes are watched in the field of the microscope. As to the first reason, the probable greater ease of procuring botanical material, the probability was not justified by the recent experience of Mr. C. H. Clark and myself at the New Hampshire College Summer School of Biology. We there went over, with nearly twenty teacher-students, the work in botany and zoology recommended in the programs of the Natural History Conference Committee, the afternoon sessions being devoted to botanical instruction by Mr. Clark, and the morning sessions to zoological instruction by myself. We both spent much of our spare time foraging for supplies, but I think Mr. Clark had the more difficult task of the two. Evidently these reasons are open to question, and, in any event, as mere reasons of expediency, they should give way to the larger considerations involved in other phases of the subject.

The limits of time forbid present discussion of the many claims of biology as a whole upon modern education, but I may say in passing that one of the most important of these claims is to be found in the relations of biological science to the philosophical problems of the day. Our philosophy is so permeated with the evolutionary phraseology that a knowledge of biological terms and processes is essential to the daily reading of an intelligent man. Such knowledge cannot be adequately obtained from the study of either plants or animals alone.

I believe that the position of a large proportion of biological teachers in America concerning biology in the high schools may fairly be represented by the following propositions: (1) That biology should be taught rather than either botany or zoology alone; (2) That the course should cover two years of at least three periods a week if possible, if not, that it cover as much time as can be spared to it, the minimum being one year; (3) That in general the time should be about equally divided between animals and plants, and that the study of the latter should come first, although some simple animal cells may well be studied at the start in connection with the lowest plants; (4) That the instruction should be given by means of the laboratory method of individual study of organic types, beginning with

the lower forms and proceeding upward in the scale of life; (5) That the methods employed should aim to develop the faculties of the student as well as to add to his store of knowledge—should be educative as well as instructive; and (6) That the laboratory work should be supplemented to as great an extent as possible by field excursions and outside reading.

It is scarcely necessary at this time to emphasize the importance of the laboratory method of studying biology. It is the only possible way; and if it cannot be adopted the boys had better be turned out in the woods to study nature first hand there. They will thus gain more useful knowledge and experience than they possibly could from the old-fashion textbook of zoology in which the student was introduced through a dead language to a much deader world. The equipment of a biological laboratory need not be very expensive. The essential furniture will consist of low simply-constructed tables with accompanying chairs, shelf-room and window-space. Each student should be provided with a compound microscope which can be purchased for $17.00, and a few simple accessories. Glass jars of some form—nests of beakers of larger sizes are excellent—should be provided for aquaria, and some simple reagents and dissecting dishes are necessary.

The logical method of commencing the study of zoology unquestionably is to study the lowest forms first and proceed in natural sequence to the higher ones. The student thus acquires a philosophic view of the animal kingdom and of the method of its development. He studies first the cell in the manifold modifications which it assumes in the one-celled animals; then he sees cells remaining connected superficially to form the simplest metazoa, and finally studies their myriad combinations in the higher animals. He proceeds from the simple to the complex—studies the materials of construction before studying the completed structure. The chief objection that has been raised to this method is that the student is required to begin the subject with high powers of the microscope —an instrument with which he may not be familiar—and that by means of it he is suddenly introduced to new and strange forms of life. This objection has been urged with

force by the master-teacher of modern biology, Professor Huxley, who, in the revised edition of his Course in Practical Biology, begins with the frog and works downward. That the experience of American teachers does not lead them to attach so much importance to the objection is shown by the fact that all of the authors of our best laboratory manuals—such as those of Dodge, Bumpus, Brooks and Boyer—have adopted the method of proceeding from below upward, and I think the practice of a majority of biological teachers points in the same direction. Possibly the aptness of American boys and girls in mastering such details as those of microscopic technique may account for the difference in the practice.

A serious objection to beginning the study of zoology with the frog or any higher animal is that it involves putting the student to the work of dissection before his interest is aroused. To many boys and more girls this is sufficient to give them a dislike to the whole subject. But if they first study living animals by watching their movements beneath the microscope, their interest can be so aroused that they can be led to simple dissections without difficulty. Many of them, indeed, will be so charmed with the work that they will echo the sentiment of the young lady at a leading New England college who is credited the enthusiastic remark that "Earthworms are perfectly lovely, especially the inside."

The teacher should adopt one of the newer laboratory guides, selecting the one that seems best adapted to the needs of the class and the time to be given to the subject, and having devoted a preliminary exercise to the use of the microscope, should start the students in individual studies of the types treated of in the guide. Abundance of material should be provided, and the students should be taught to rely upon their own resources to as great an extent as possible. At first they will need constant assistance, but later they will become more independent. Drawings and full notes are to be required.

An important part of the educational value of a laboratory course in biology depends on the requirements as to the student's notes. If one adopts the somewhat common practice of

allowing the student to follow the laboratory guide in his notebook, often answering questions by number with a yes or no, the results will be far from satisfactory. In my own classes I have adopted the method of writing upon the blackboard a definite subject, *e. g.*, "A Description of the Structure and Biology of the Amoeba," upon which I require an original essay embodying the results of the student's observations, and such additional explanations as I have given the class at the time the animal was studied. These essays are written upon one side of the letter size paper that goes between clip binders. The drawings are incorporated in the proper places with explanations beneath, the aim being to make all as clear and concise as it should be in a book. These essays are submitted once a week, and if not satisfactory are rewritten. I hope soon to arrange a coöperation with the English department so that the essays may count as English exercises and be reviewed from the rhetorical point of view. Very decided progress has resulted from this method which seems to me the most desirable mode of note-keeping in such laboratory work.

But the ordinary laboratory manual by no means includes all of the "pedagogical contents of zoology." In general it confessedly covers with fair completeness only the morphological side of the subject and leaves almost or quite untouched important phases of the science which should never be ignored. To guide a student along the morphological road is unquestionably the safest and surest way of leading him to a sound basis of biological knowledge, but every opportunity should be taken to point out to him the objects of fascinating interest that are found beside the way. Failure to do this leads to the production of those near-sighted naturalists, who, in the expressive words of Professor Forbes, " must have nature boiled in corrosive sublimate solution and fried in paraffine and sliced by a microtome before they care for it." These are not the nature students the high schools wish to produce. Broadness, not narrowness, is here the aim; and the results in this respect will depend largely on the culture, enthusiasm and preparation of the teacher.

The most important general result to be taught in connec-

tion with morphology is that of physiology. So far as possible the study of function should coincide with the study of form. To a considerable extent the newer laboratory manuals provide for this, especially in the lower groups of animals. Emphasis should be laid upon this side of the subject, and explanations be reiterated until the student masters each detail. In the same connection—and here is one of the most important phases of zoology—the teacher should develop those laws of life which give to biology its greatest interest, such as the law of the physiological division of labor and of structural progress from simple to complex ; the relation of the one-celled animal to the multicellular one; the similarity of individual development to that of the group; the significance of the nucleus; the phases of reproduction; the facts of biogenesis and abiogenesis, of homogenesis and heterogenesis; the relations of parasitism to degeneration; the differences between plants and animals; the infinity of variations; the main facts of mimicry and protective resemblance; the effects of heredity and environment; the elements of natural selection, and an outline of the theory of organic evolution.

Perhaps you think this is laying too great a burden upon the teacher: it need not, for he may find an admirable, though concise discussion of these principles in Parker's Elementary Biology, and a more elaborate account of many of them in Lloyd Morgan's Animal Life. He should also have at hand for familiar reference Wallace's Darwinism, Poulton's Colors of Animals, Beddard's Animal Coloration, Rolleston's Forms of Animal Life, the Standard Natural History, the important zoological text-books, and as many other similar works as possible.

Perhaps the next most essential feature of the zoological course is a knowledge of the main outlines of animal classification. Not many years ago zoology was taught as if it consisted only of classification, and the inevitable reaction has gone so far that at present there is a tendency to ignore it altogether. This, however, is to be deplored. Classification is an essential feature of the science and should receive due consideration. Here the safest guide for the American teacher is

the Standard Natural History which should be in every school library as a work of reference.

Much can be done in arousing the student's interest by means of field excursions and outside reading. These excursions should be taken as frequently as they conveniently can be, and be under the personal supervision of the instructor. Inland schools should plan, if possible, at least one trip to the seaside, choosing a time when the tide will be out during the visit, where crabs, sponges, starfishes, sea-urchins and anemones may be studied as well as sea-lettuce, rock-weeds and many other forms of plant and animal life.

The amount of collateral reading that may be done will vary with the conditions of the school and the interests of the individual student. Biology opens to one an enormous field of literature of fascinating interest in which the teacher should always be browsing; and if wise he will lead his flocks to the feet of the master-minds who have ever found joy and inspiration in the green pastures and beside the still waters, where dear old Mother Nature is always ready to receive our worship and breathe a benediction upon our holiest aspirations.

New Hampshire College, September, 1894.

EDITORIALS.

—THE International Geological Congress met at Zurich, commencing on August 29th, and continuing until September 1st, inclusive. On the third of September the Congress started on an extended excursion through the Alps for examination of the geological features en route. Numerous important papers were read, but no official expression as to rules or modes of proceedure in geology were issued or discussed. The most important proposition in this direction had reference to the organization of the congress itself. Dr. Fraser of this city offered the following resolution, "with reference to the organization of the next congress." "(1) To what extent does the Congress recognize the right of Government bureaus as such, or of societies, or any other organization, to send delegates to the congress? (2) Within what limits does the Congress recognize the right of these representatives, or of a part only of the members of the Congress which come from the same country, to designate the Vice-President representing their country, or to act without coöperation with their compatriots in the Congress.?"

This resolution was rendered necessary by the arbitrary action of the president Prof. Renevier, in electing as Vice-President representing the United States, a person who was not présent, but who had been recommended for the place by letter. The person so elected is a member of U. S. Geological Survey, and although this fact could not debar him from the position, his appointment under such circumstances brought into prominence the question as to the relative claims of various bodies to appointment to the official positions in the Congress. Since the Committee which originally represented the United States was driven out of existence, owing to the contributive neglect of some of its members, this country has no official representation in the Congress. Hence the propriety of the resolution offered by Dr. Fraser. An easy solution of the question would appear to be suggested by the language of the resolution. That is that the members in attendance from a given country, should get together in advance, and nominate their candidates for presentation to the congress.

—IT is proposed by the Filson Club of Louisville, Kentucky, to publish a work on the life and writings of Constantine Samuel Rafinesque by the well known zoologist, Dr. R. Ellsworth Call. An extract from the preface says:

"This memoir had its inception in an attempt to clear up certain matters connected with the synonymy of a large and important group of fresh-water mollusks—the Unionidæ. A number of very remarkable facts connected with the personality of its subject were thus incidentally learned. As the collation of data proceeded, the facts learned seemed of sufficient importance to group them for presentation to the literary and scientific world in the hope that a better and more intelligent understanding of the work of this eccentric naturalist might result. A number of impressions were forced upon my attention as the work proceeded; among other conclusions reached, was the one that Rafinesque had not been always fairly treated by his cotemporaries. Resulting from this was the conviction that many naturalists now living have formed opinions concerning the nature and value of Rafinesque's work which appear to me to be quite erroneous. In the hope that some of these misapprehensions might be corrected, the task of writing his life, which is quite a labor of love, was undertaken."

The prospectus goes on to say "the publication will be in the sumptuous quarto form adopted by the Filson Club, and issued in paper only. It will contain several full page illustrations, one of which will be a portrait of its subject. A complete bibliography of the writings of Rafinesque on every subject, comprising over four hundred titles, will be included, together with a certified copy of his will, one of the most remarkable testamentary documents ever probated," etc.

The gentlemen engaged in this enterprise probably think that they are conferring a benefit on contemporary and future science by issuing this publication. We wish to state that in our opinion the money devoted to it might be expended in a much more profitable direction. A reprint of Rafinesque's botanical and zoological papers, so that they can be made accessible to students, would be far more useful to science, and we are glad to notice that the same publishers (Jno. P. Morton & Co.) propose to issue a reprint of the Ichthyologia Ohiensis. We do not mean to intimate, in making this suggestion, that the works of Rafinesque ever had more than a very moderate scientific value, but he has added so much to the nomenclature that it ought to be possible to refer easily to them, whereas now many of them are inaccessible to most naturalists.

Rafinesque is well known as a most careless writer who inflicted endless difficulties on his successors. Some of the matter of his papers is fictitious, and much of it of such an indefinite character that it should not be admitted into scientific literature. Some naturalists have been at great pains to identify his species, but such identifica-

tions will be ultimately set aside, when a more critical spirit prevails among species zoologists. Money is so badly needed for scientific research and its publication, that it is melancholy to notice its perversion to such an object. It is also difficult to understand how any one who understands the true needs of science can devote his time to writing such a book. In concluding these remarks, we wish to emphasize the fact that Mr. Rafinesque was not a Kentuckian, nor an American, so that patriotic (!) motives can scarcely enter into the proposition.

—It is greatly to be hoped that the newly established Botanical Society of America can be induced to hold at least some of its meetings at the same times and places as the societies of Naturalists, Morphologists and Physiologists, for not a few will be members of more than one of these organizations, while there are many questions like those of evolution, heredity, geographical distribution, studies of the cell and of protoplasm, which, whether presented from the zoological or the botanical side are of equal interest to all. We notice that the provisions of the constitution of the new society are in effect that annual and special meetings are to be held at times and places appointed by the council, so that there is, in this respect, no trouble in affiliation with the older organizations.

—Professor W. W. Norman of De Pauw University goes to the place in the University of Texas recently vacated by C. L. R. Edwards, now of Cincinnati. In view of the treatment experienced by Dr. Edwards, the position can hardly be said to be a desirable one, and we withhold our congratulations until we see whether the university authorities know more or are more sensible of the advances of science than they were a few months ago. The best we can do is to extend our sympathies.

The numbers of the American Naturalist for 1894 were issued at the following dates: January, Jan. 25; February, Feb. 17; March, Mch. 8; April, Apl. 2; May, May 4; June, June 1; July, July 13; August, Aug. 14; September, Sept. 15; October, Oct. 10; November, Nov. 8; December, Dec. 5.

RECENT BOOKS AND PAMPHLETS.

ALLEN, H.—A Monograph of the Bats of North America. Bull. No. 43, U. S. Natl. Mus. From the Smithsonian Institution.

ALLEN, J. A.—Cranial Variations in *Neotoma micropus* due to Growth and Individual Differentiation.——Remarks on specimens of *Chilonycteris rubiginosus* from Western Mexico, and on the Color Phases of *Pteronotus davyi* Gray. Extr. Bull. Am. Mus. Nat. Hist., Vol. VI, 1894. From the author.

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RECENT LITERATURE.

Seitaro Goto.—Studies on the Ectoparasitic Trematodes of Japan.[1]—This volume forms one of the most important pieces of work which has ever been written on the ectoparasitic trematodes, and is the result of about four years of careful and exact study. In the first part (176 pgs.) of the work, the author treats the anatomy in detail; then follow several pages of biological notes, a detailed account (pp. 182–253) of the classification, analytical key (pp. 254–261) to genera and species described, bibliography (pp. 262–267), and 27 finely drawn and well executed plates. Revised generic and specific diagnoses are given, together with a historical review of the different genera.

The following genera and species are described:—

I. MICROCOTYLE B. & H., 1863:—
1. *M. caudata* n. sp., gills of *Sebastes* sp. sp.;
2. *M. sebastis* n. sp., gills of *Sebastes* sp. sp.;
3. *M. elegans* n. sp., gills of *Scombrops chilodipteroides;*
4. *M. reticulata* n. sp., gills of *Stromateus argenteus;*
5. *M. truncata* n. sp., gills of *Pristipoma Japonicum;*
6. *M. fusiformis* n. sp., gills of *Centronotus nebulosus;*
7. *M. chiri* n. sp., gills of *Chirus hexagrammus;*
8. *M. sciænæ* n. sp., gills of *Sciæna sina;*

II. AXINE Abildg., 1794:—
9. *A. heterocerca* n. sp., gills of *Seriola quinqueradiata;*
10. *A. aberrans* n. sp., gills of *Belone schismatorhynchus;*
11. *A. triangularis* n. sp., gills of *Anthias Schlegelii;*

III. OCTOCOTYLE Dies., 1850:—
12. *O. major* n. sp., gills of *Scomber colias;*
13. *O. minor* n. sp., gills of *Scomber colias;*

IV. DICLIDOPHORA Dies., 1850:—
14. *D. smaris* Ijima MS., mouth-cavity of *Smaris vulgaris*, on caudal segment of a Cymothoa;
15. *D. elongata* n. sp., mouth-cavity of *Pagrus tumifrons*, occasionally on the Cymothoa in the oral cavity;
16. *D. sessilis* n. sp., oral cavity of *Choerops Japonicus;*
17. *D. tetrodonis* n. sp., gills of *Tetrodon* sp., sp.;

V. HEXACOTYLE Blainv., 1828:—
18. *H. acuta* n. sp., gills of *Thynnus sibi;*
19. *H. grossa* n. sp., gills of *Th.* sp.;

VI. ONCHOCOTYLE Dies., 1850:—
20. *O. spinacis* n. sp., gills of *Spinax* sp.;

VII. CALICOTYLE Dies., 1850:—
21. *C. Mitsukurii* n. sp., cloaca of *Rhina* sp.?;

VIII. MONOCOTYLE Tschbrg., 1878:—
22. *M. Ijimae* n. sp., oral cavity of *Trygon pastinaca;*

[1] Journ. College of Science, Imp. Univ., Tokyo. Vol. VIII, Part I, 1894, 273 pgs., 27 plates.

IX. EPIBDELLA Blainv., 1828: —
23. *E. Ishikawae* n. sp., gills of *Lethrinus* sp.?;
24. *E. ovata* n. sp., gills of *Anthias Schlegelii;*
X. TRISTOMUM Cur., 1817: —
25. *T. sinuatum* n. sp., gill-plates of *Histiophorus* sp.;
26. *T. ovale* n. sp., oral cavity of *H. orientalis*, H. sp., and? *Cybium;*
27. *T. rotundum* n. sp., gills of *Xiphias gladius;*
28. *T. foliaceum* n. sp., gills of gen. sp. (Japanese Hazara);
29. *T. Nozawae* n. sp., fins of *Thynnus sibi;*
30. *T. biparasiticum* n. sp., carapace of a copepod (*Parapetalus*) and gills of *Thynnus albacora.*

It is somewhat striking that of all the thirty Japanese species described, the author does not consider a single one identical with any forms heretofore mentioned, but when one looks at the magnificent anatomical work contained in this volume he certainly feels very far from calling specific determinations into question.

Several points in Goto's interpretation of anatomical and histological structures are worthy of special notice: —

1. The prismatic, refractive fibres, which constitute the wall of the suckers in the genera *Axine, Microcotyle, Octocotyle, Diclidophora, Hexacotyle* and *Onchocotyle*, are usually looked upon as muscular fibres, but Goto agrees with Wright and Macallum (in *Spyranura*) in considering these fibres more of a non-contractile supportive, connective tissue nature.

2. The penis "is to be regarded as formed by an elevation of the wall of the genital atrium around the opening of the vas deferens and a simultaneous displacement of the latter from the base of the penis towards its top; so that the cavity of the penis is morphologically speaking as much the external surface of the body as the genital atrium, and the prostate glands are therefore to be regarded as a special modification of the dermal glands,—a view clearly in accordance with some facts observed [by Haswell] in Temnocephala."

3. Agreeing with Looss, Goto considers the vagina of the Cestoda homologous with the uterus of the Trematoda. Laurer's canal of the Digenea is homologized with the genito-intestinal canal of the ectoparasitic Trematoda, the receptaculum vitelli of *Aspidogaster* and the "anterior blind vagina" of *Amphilina.* While Looss looks upon the uterus of the Cestoda as homologous with the Laurer's canal of distomes, Goto homologizes the uterus of the Cestoda with the vagina of the monogenetic Trematoda. These homologies are discussed at length and are diagrammatically figured on Pl. XXVII.

For important and interesting discussions of other histological and anatomical structures we must refer to the original work.

C. W. STILES.

General Notes.

PETROGRAPHY.[1]

Composite Dykes on Arran.—Professor Judd[2] describes a number of "composite" dykes on the Island of Arran, in which the well-known "Arran pitchstone" and a glossy augite-andesite occupy different portions of the same fissure, either rock appearing in the center of the dyke, with the other on one or both of its peripheries, or the one rock cutting irregularly through the other. The relations of the rocks indicate that there was no regular sequence in the intrusion, the pitchstone having been intruded sometimes before, sometimes after the andesite. Each rock contains fragments of the other (in different dykes), and the two rocks are always separated by a sharp line of demarkation. The andesite is a basic rock containing about 56 per cent of silica, while the pitchstone is a pantellerite with 75 per cent of SiO_2 or an augite-enstatite-andesite with 66 per cent of SiO_2 and 4.13 per cent K_2O. The andesite is well characterised. It passes into a tholeite with intersertal structure, by a decrease in the glassy component, and upon further loss of glass it passes into diabase. The pitchstone is largely an acid glass, surrounding crystals of quartz, and microlites of augite, feldspar, magnetite, etc. The author adds to the list of individualized components already known to exist in the rock hyalite and tridymite. The latter mineral occurs in plates aggregated into spherules and globules that surround quartz crystals, and the hyalite forms globules scattered here and there through the glass. The author thinks that materials of such widely different nature as that existing in these dykes could not have been formed by the differentiation of a magma after its intrusion into the dyke fissures, but that the differentiation must have taken place while the magma was still in its subterranean reservoir.

Analyses of Clays.—Hutchings[3] quotes a series of analyses of carboniferous clays to show that these substances possess the requisite composition to become clay slates upon compression. He ascribes the small percentages of alkalies shown in most clay analyses to the fact

[1] Edited by Dr. W. S. Bayley, Colby University, Waterville, Maine.
[2] Quart. Jour. Geol. Soc., xlix, 1893, p. 536.
[3] Geol. Magazine, Jan. and Feb., 1894.

that these analyses are of commercially valuable clays, selected for their small alkali contents. In the course of his article the author corrects some of the statements made in earlier papers and amplifies others. He declares that newly formed feldspar is present in the slates metamorphosed[4] by the shap granite and in other contact slates. In the spots of the shap rocks, and in those of other contact slates, there is always present, in addition to its individual components, more or less of a yellowish-green very weakly polarizing substance in which the other components of the spot are imbedded. This is believed to possess an indefinite composition, and to be the result of aqueo-fusion of some of the constituents of the original rock and the solidification of the product in an amorphous condition. The paper concludes with a statement of the author's views concerning the transformations that rutile, biotite, quartz, feldspar, cordierite and other contact minerals undergo in cases of contact metamorphism.

The Phonolites of Northern Bohemia.—The phonolites of the Friedländer district of North Bohemia are nosean bearing trachytic phonolites and nepheline-phonolites, according to Blumrich.[5] The latter contain phenocrysts of anorthoclase in a groundmass of sanidine, nepheline and aegerine crystals and groups of a new mineral which the author calls hainite. This hainite is a strongly refracting but a weakly doubly refracting colorless substance. It occurs in tiny triclinic needles with a density of 3.184. These unite into groups. It is found also as well-developed wine-yellow crystals forming druses in cavities in the rock. The mineral has a hardness of 5, and it is optically positive. It is supposed to be closely related to rinkite, hjortdahlite and the other fluorine bearing silicates common to the eleolite-syenites. In addition to hainite the druse cavities contain albite, chabazite and nosean. In the trachytic phonolites a glassy base was detected.

Spherulitic Granite in Sweden.—Loose blocks of spherical granite are reported by Backström[6] from Kortfors, in Orebro, and Balungstrand in Dalekarlien, Sweden. The rock from Kortfors is a hornblende granitite containing concentric nodules composed of four zones. The inner one consists of oligoclase, microcline and quartz; the second of oligoclase in radial masses and small quantities of hornblende, biotite, magnetite, orthoclase and quartz; the third of hornblende, biotite, oligoclase and a little biotite, and the peripheral zone

[4] Cf. American Naturalist, 1892, p. 245.
[5] Min. u. Petrog. Mitth., xiii, p. 465.
[6] Geol. Foren. i. Stockh. Förh. 16, p. 107.

of magnetite in a matrix of oligoclase. The structure of the spheroids, with the younger minerals nucleally and the older ones peripherally distributed, indicates to the author that they were produced by liquation processes. The rock from Balungstrand possesses a coarse groundmass consisting almost exclusively of microcline and quartz. The spheroids are essentially oligoclase spherulites peripherally enriched by biotite. They are clearly older than the groundmass.

Diabase and Bostonite from New York.—A few dyke rocks cutting the gneisses of Lynn Mountain, near Chateaugay Lake, Clinton Co., N. Y., are described by Eakle[7] as consisting of olivine diabase and of bostonite. The latter rock is porphyritic with phenocrysts of red orthoclase in a fine-grained groundmass with the trachytic structure. It differs from the other bostonites of the region in the presence of much chloritized augite in its groundmass. It is also more acid than these. Its analysis gave:

SiO_2	Al_2O_3	Fe_2O_3	CaO	MgO	K_2O	Na_2O	Loss	Total
67.16	14.53	4.17	1.26	.41	6.10	5.55	1.10 =	100.28

The olivine diabase differs from the ordinary ophitic diabases in that much of its augite is in idiomorphic forms. They thus resemble Kemp's augite camptonites.

Petrographical News.—A very interesting series of analyses of rocks from the central and northeastern portions of the Mittelgebirge is given by Hibsch.[8] The series includes analyses of phonolites, dolerites, camptonites, nepheline and leucite tephrites, augitites and basanites. Many of the rocks have been described in the literature.

Cohen[9] has obtained from the Transvaal, Africa, specimens of a calcite bearing aplite and of a melilite augite rock of a somewhat abnormal character. The aplite is from the mine of the Iron Crown Gold Mining Co., near Hamertsburg, and the melilite rock from near Palabora. The melilite rock is a fine-grained aggregate composed largely of honey-yellow melilites and black augites. On its druse walls are little crystals of the first-named mineral, and through the druse cavities extend thin plates of copper. In the thin section, clear, colorless melilites, with rounded outlines and olive-green grains of augite are seen to lie in an opaque granular groundmass in which are dots and flakes of copper.

[7] Amer. Geologist, xii, p. 31.
[8] Min. u. Petrog. Mitth., xiv, p. 95.
[9] Minn. u. Petrog. Mitth., xiv, p. 188.

Backström[10] fused feldspathic phonolite and obtained as the product upon cooling a colorless glass filled with microlites of oligioclase, nepheline, small microlites of colorless pyroxene and tiny grains of picotite and olivine (?). Upon fusing a leucite phonolite, containing nosean, SO_3 is driven off and the resulting product is a glass enclosing microlites of oligoclase, a few prisms of nepheline and abundant crystals of a yellow pyroxene with the properties of aegerine.

[10] Bull. d. l. Soc. Franc. d. Min., 1893, xvi, p. 130.

GEOLOGY AND PALEONTOLOGY.

Ancient Conglomerates.—The presence of intra-formational conglomerates is a not uncommon phenomenon. Dr. Walcott notes several localities where this form of conglomerate occurs in Paleozoic limestone formations, and describes typical ones found in Vermont and New York, Pennsylvania, Virginia and Tennessee. The author defines this species of conglomerate as one formed within a geological formation of material derived from and deposited within that formation. As to their origin, he offers the following theory. Low ridges or domes of limestone were raised above the sea level and were subjected to the action of sea shore ice and the aerial agents of erosion. In the intervening depressions of these ridges calcareous mud was being deposited which was solidified soon after deposition. The material forming the conglomerate was transported from the shore line and dropped upon the sea bed by floating ice. The facts from which these inferences are drawn are given in detail. (Bull. Geol. Soc. Am., Vol. 5, 1894).

Subterranean Waters on the Coastal Plain.—N. H. Darton has published a brief review of the geological conditions under which subterranean waters occur in the Coastal Plain region of the middle Atlantic slope, together with an account of wells bored. He shows that the geological relations are favorable to the wide circulation of waters at several horizons, and gives the approximate vertical positions and general areal distributions of these horizons. In southern New Jersey, Delaware and a portion of Maryland, the sand series of the Chesapeake formation are the principal water producers. Along the western edge of the Coastal Plain from Petersburg to Staten Island, the basal members of the Potomac formation yield water at moderate depths. The author gives also the "prospects" in several districts. About Norfolk, water will probably be found on the crystalline floor, 1,500 feet below the surface; in the peninsula region of eastern Virginia and Maryland, at the base of the Chesapeake beds at depths varying from 100 to 400 feet; on the "eastern shore" of Maryland there are many favorable prospects, successful wells being in operation, drawing their supplies from the Chesapeake, 200 to 300 feet below the surface, and from the Pamunkey sands, reached by 350, 440 and 910 feet boring. (Trans. Am. Inst. Mining Eng., 1894).

The Shasta-Chico Series.—The protracted investigations of Messrs. Diller and Stanton concerning the Cretaceous formations of western United States result in an accumulation of data on which are based a number of interesting conclusions. The Knoxville, Horsetown and Chico beds of northern California and Oregon are found to be continuous series of deposits and the authors accordingly propose for them the name Shasta-Chico series. The Wallala beds represent a phase of the Chico. The Mariposa and Knoxville beds are distinct faunally and are unconformable. Ther former is Jurassic, the latter Cretaceous. The attentuation of the Shasta-Chico series westward from the Sacramento Valley and the overlapping of the newer beds upon the older crystalline rocks of the coast range shows that the coast range was formed before the deposition of the Shasta-Chico series, and probably at the close of the Jurassic when the Mariposa beds were upturned.

The subsidence of the whole Pacific coast from Alaska to Mexico is shown by the successive peripheral attenuation of the lower beds and the landmark overlapping of the upper ones. The subsidence was probably not uniform throughout the whole region.

The final folding of the Sierra Nevada rocks and an uplifting of the range occurred at the close of the Jurassic.

The Shasta-Chico series represents the Cretaceous time from the beginning of the Lower Cretaceous to the Middle of the Upper Cretaceous, and it may be closely correlated with the Queen Charlotte Island and Nanaimo groups.

The evidence from fossil plants indicates that the Potomac epoch is included in that represented by the lower part of the Shasta-Chico series. It is also highly probable that the Comanche series of Texas and Mexico is contemporaneous with a large part of the Shasta-Chico series. (Bull. Geol. Soc. Am., Vol. 5, 1894).

A Gypsum " Cloche."—While excavating stone for plaster in the southern borders of the forest of the Montmorency à Taverny (Seine-et-Oise) a *cloche*, or natural cavity, was found in a mass of gypsum. This *cloche* is ellipsoidal in form, about 10 metres in length, and 5 to 6 metres high. The top of the cavity presents the peculiar appearance resulting from the slow dessication of the homogeneous rock. The sides are polished, with the edges of all the angles rounded off. The floor is an irregular heap of gypsum blocks of various sizes. Certain parts of this cavity are lined with small gypsum crystals.

That the cavity is the result of the action of water is undoubted, and three hypotheses are given as to the manner of erosion. (1) The water may have entered from above or laterally and slowly dissolved the gypsum. (2) The water may have entered from below through a fissure acting as a natural siphon. (3) There may exist, beneath the mass exploited, a subterranean stream flowing over a second deposit of gypsum. The second mass having been dissolved and carried away by the water would leave a cavity into which the first mass would fall. The cavern thus formed would fill with water percolating through the fissures, from which would result the phenomena of solution and curious recrystallization of gypsum observed on the roof and sides of the *cloche*. (Feuille des Jeunes Naturalitses, no date).

The Malaspina Glacier.—The term Piedmont has been applied to glaciers formed on comparatively level ground at the bases of mountains where the ice is not confined by highlands. They are fed by Alpine glaciers which spread out and unite with each other on leaving the valleys through which they descend from snow fields at higher elevations. The only known example of this class is the Malaspina glacier which occurs in Alaska, on the plain intervening between the Mt. St. Elias range and the ocean. A detailed description of this phenomenon by I. C. Russell was recently published, of which the following is an abstract.

The Malaspina glacier extends westward from Yakutat Bay for 70 miles, with an average breadth of 20 to 25 miles. It is a nearly horizontal plateau of ice. The general elevation 5 or 6 miles from its outer border is about 1,500 feet. It consists of three lobes, each of which is practically the expansion of a large tributary ice stream. The largest has an eastward flow toward Yakutat Bay, and is fed by the Seward glacier. It ends in a low frontal slope, while the southern border skirts the coast and forms the Sitkagi bluffs. The middle lobe is the expanded terminus of the Agassiz glacier flowing toward the southwest. This lobe is complete, and is fringed all about its outer border by broad moraines. The third lobe results from the union of the Tyndall and Guyot glaciers; it has a general southward flow and pushes out into the ocean, breaking off forms of magnificent ice cliffs.

On the north border of the glacier the surface-melting gives origin to hundreds of rills and rivulets of clear sparkling water which course along in channels of ice until they meet a crevasse or moulin and plunge down into the body of the glacier to join the drainage beneath. In the southern portion of the glacier abandoned tunnels 10 to 15 feet

high made by englacial streams are sometimes revealed. The rapid melting of the surface ice produces curious phenomena. Where the ice is protected by belts of stone and dirt from the action of sun and air, the adjacent surface wasting away leaves ridges, while large isolated stones give rise to pinnacles and tables, but smaller ones, especially those of dark color, cause depressions.

The great central area of the glacier is composed of clear white ice which is bordered on the north by a broad, dark band of boulders and stones. Outside of this, forming a belt, concentric with it, is a forest covered area, in many places four or five miles wide. The forest grows on the moraine which rests upon the ice of the glacier.

The Malaspina glacier, in retreating, has left irregular hillocks of coarse débris which are now densely forest-covered. These deposits do not form a terminal moraine, but a series of irregular ridges and hills with a somewhat common trend. They indicate a slow general retreat without prolonged halts.

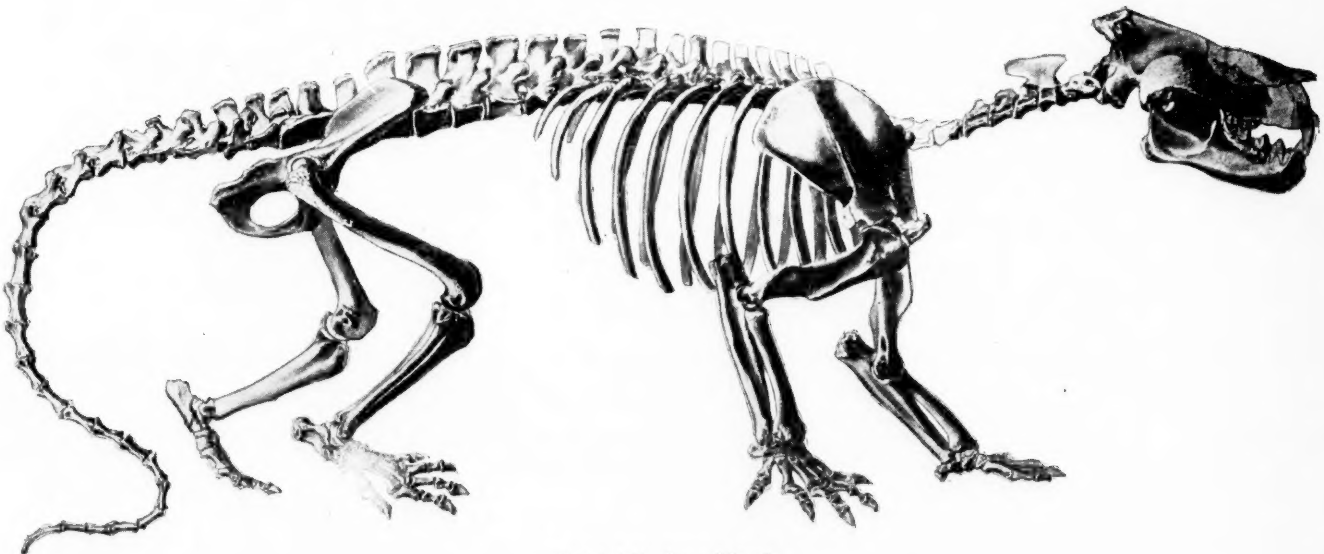
The outer portion of the barren moraine and the forest covered area characterized by innumerable lakelets from 100 feet to 200 yards across. They are generally circular and have steep walls of dirty ice which slope toward the water at high angles. Their presence in large numbers indicate that the ice must be nearly or quite stationary, otherwise the basins could not exist for a series of years.

On the west and north sides of the Chaix hills several typical "marginal lakes" occur similar to the well known Merjelen See of Switzerland.

The drainage of the Malaspina glacier is englacial or subglacial. Along the southern margin hundreds of streams pour out of the escarpment formed by the border of the glacier, or rise like fountains from the gravel accumulated at its base. All are brown and heavy with sediment. The most remarkable of these springs is Fountain Stream. It comes to the surface through a rudely circular opening, nearly 100 feet in diameter, surrounded in part by ice. Owing to the pressure to which the waters are subjected they boil up violently, and are thrown into the air to the height of 12 or 15 feet and sends jets of spray several feet higher. The waters rush seaward in a roaring stream 200 feet broad which soon divides into many branches, spreading a sheet of gravel and sand right and left into the adjacent forest.

About the southern and eastern borders of the glacier osars and alluvial cones abound. It is in this region that the ideal conditions for these formations exist. Here the ice sheet is stagnant on its border, and is retreating; it rests on a gently inclined surface, higher on

PLATE XXXIII.



Patriofelis ferox Marsh.

the southern margin than under its central portion, with high lands on the upper border from which abundant débris is derived.

There has been a recent advance and subsequent retreat of the glacier on its eastern margin. During its advance it probably extended to the ocean. There are several indications that the coast in the vicinity has been rising and that the process is still continuing.

Plistocene Problems in Missouri.—The three hypotheses as to the origin of the Boulder Drift and Loamy Clay in Missouri, north of the Missouri River, are briefly styled by J. E. Todd, the subglacial, the lacustrine and the fluviatile. The objection to the first is the great difference in altitude of the drift in Missouri and that in Illinois not fifty miles away, together with the absence of drift over Saint Louis County and down the valley of the Meramee, and also the apparent impossibility of the land ice reaching central Missouri without overflowing the Wisconsin driftless area. To the second and third hypotheses are opposed the nature of the deposits and the great width and depth of the troughs of the Missouri and the Mississippi Rivers. Todd confines himself to stating the problems without advancing any theory of explanation. Further research, he thinks, may remove the objections he finds in the last two and it is not improbable that the deposits may be accounted for by a combination of the lacustrine and fluviatile theories. (Bull. Geol. Soc. Am., Vol. 5, 1894).

Wortman on the Creodont Patriofelis.—Dr. J. L. Wortman has published, in the Bulletin of the Amer. Museum Nat. History of New York, a study of a remarkably perfect skeleton of the *Patriofelis ferox* Marsh, which he found in the Bridger beds of S. W. Wyoming. The species was described by Marsh under the name *Limnofelis ferox.* Limnofelis Marsh is shown, by the material described, to be synonymous with Patriofelis Leidy of earlier date, and Protopsalis Cope of later date turns out to have been founded on a species of the same genus. Wortman remarks of the genus: "The larger species, *P. ferox,* is one of the largest Credonts known, and equalled in size a full-grown black bear. The head was disproportionately large and massive, almost equalling in this respect an adult lion. The smaller species, *P. ulta* Leidy, was almost one-third smaller. In both there were a long and powerful tail, and broad plantigrade feet, which, together with other characters presently to be considered, lead to the conclusion that they were aquatic in habit."

As regards the systematic position of Patriofelis, Wortman says: "Its general skeletal structure is so much like Oxyæna, that notwithstanding the differences in the teeth they must be placed in the same family. Oxyæna is the older form and has the more primitive dentition but the differences are not greater than we would lead to anticipate in the ancestral genus. I think that it can be accepted as demonstrated that Patriofelis is the direct descendent of Oxyæna, which may likewise have given off a branch which terminated in the modern seals. It is somewhat doubtful whether this branch leads through Patriofelis." Concerning the habits of the Patriofelis, Wortman remarks: "From the structure of the limbs more than any other feature, I am led to conclude that it was aquatic or semiaquatic in its habits. The broad, flat plantigrade feet with their spreading toes suggest at the first glance their use for swimming. The eversion of the feet, together with the general clumsiness of the limbs, point, moreover, to the fact that the animal was not an active runner. Now, if the animal was aquatic, what was the nature of its food? It certainly could not have been fish, for the reason that the remains of fishes are very scarce in the Bridger sediments. If, however, we can form any judgment from their remains, the Bridger lake literally swarmed with turtles, and if Patriofelis frequented the water, it is highly probable that they formed a staple article of its diet."

Through the kindness of the American Museum authorities, we are able to give a figure of the restoration of the *P. ferox* (Plate XXXIII) which accompanies Dr. Wortman's article.

Geological News. CENOZOIC—The fossil flora collection from Herendeen Bay, Alaska, embraces 115 forms. These forms, according to Prof. Knowlton, are so closely related to those of Greenland, Spitzbergen and the Island of Sachalin that without doubt they grew under similar conditions and were synchronously deposited. The author agrees with Sir Wm. Dawson in regarding these floras of Eocene age rather that Miocene to which they have hitherto been referred. (Bull. Geol. Soc. Am., Vol. 5, 1893).

Prof. O. C. Marsh has recently given a brief description of a phalange of a large bird which was found in the Eocene of New Jersey. This is an interesting discovery. Unfortunately Prof. Marsh gives it a new specific and even a new generic name. As neither species nor genus can be recognized from a phalange, these names constitute an unnecessary addition to the waste basket of scientific literature.

Prof. Shaler offers additional evidence of orogenic action in producing the folds of the Cretaceous and early Cenozoic beds on the Island of Martha's Vineyard, Mass. As to the origin of those movements, the author inclines to the hypothesis that transfers of sediment tend to excite mountain building action. The exposures at Gay Head and elsewhere show that a great mass of sediment accumulated in that area in a brief period, and the orogenic movements of southeastern Massachusetts occurred shortly after this importation of detritus. (Bull. Geol. Soc. Amer., Vol. 5, 1894).

The record of striæ made by Mr. Tyrrell, during his exploration of N. W. Canada and Hudson Bay, shows that one of the great gathering grounds for the snow of the Glacial period in North America was a comparatively short distance west of the northern portion of Hudson Bay, and that from that centre the ice flowed not only towards the Arctic Ocean and Hudson Bay, but it extended a long distance westward towards the Mackenzie River, and southward towards the great plains, while Hudson Bay was probably open water. (Geol. Mag., Sept., 1894).

BOTANY.[1]

Dr. Kuntze's "Nomenclatur-Studien."[2]—Dr. Kuntze's latest contribution to the nomenclature problem is in the form of a reply to certain criticisms of Pfitzer upon his alterations of names in the *Orchidaceae*. Pfitzer's criticisms are to be found in Engler's *Jahrbuecher* XIX, 1–28. Kuntze answers him in the Bulletin of the Boissier Herbarium, II, No. 7, issued in July, 1894, in an article entitled *Nomenclatur-Studien*. While this article was provoked by the strictures of Pfitzer and deals principally with the nomenclature of the orchids, it is of especial interest to American botanists on account of some criticisms of two rules adopted in this country.

The first section of the article deals with names applied by Thouars to the orchids, which Pfitzer would reject. Dr. Kuntze discusses the matter thoroughly, although he had already gone over the ground in 1891 (Rev. Gen., II, 645–650), and certainly makes a convincing argument. In the course of his reply to Pfitzer on this point, he is led to restate his position on the question of "species-majority vs. place-priority," and to criticize the rule adopted by American botanists. This is done in the second section.

Section II, entitled "priority in place at all events and Article 55," is one of considerable importance. Dr. Kuntze in his *Codex Emendatus* (Rev. Gen., III, 1, CCCCV) proposes the following additions to article 55 of the Paris Code (I quote from his English text):

"A deviation from strict priority is necessary for genera published on the same day and united afterwards:

(1) "If they got no species at their first publication, the genus name to which in 1753 or afterwards was put the first specific name is legitimate.

(2) "If they got also their first species on the same day, the genus name having received most species on that day must be preferred". . . .

Instead of this criterion of "species-majority," American botanists have taken priority of place in the book in which both names were published. This criterion is undoubtedly simple, easy of application, and one obviating all discussions to which the application of the other might give rise. But Dr. Kuntze proceeds to make some applications

[1]Edited by Prof. C. E. Bessey, University of Nebraska, Lincoln, Nebraska.

[2] Read before the Botanical Seminar of the University of Nebraska, Sept. 22, 1894.

of the rule which, as he says, operate as a *reductio ad absurdum.* He makes a list of genera subject to the operation of the rule, taken only from Linne's *Species* of 1753, and including good sized genera only. From this list it appears that the American rule will require the use of *Phaca* instead of *Astragalus*—involving the change of 1300 names—of *Sarothra* for *Hypericum,* and of *Amygdalus* for *Prunus.* In his list, taken only from the 1753 edition of the *Species plantarum,* and not an exhaustive one, the American rule will alter the names of 20 genera and 4600 species. None of these are affected by the species-majority rule; *Phaca,* which appears on page 755 of the *Species* above *Astragalus,* has there but 2 or 3 species, while *Astragalus* has 33. So *Pirus* on page 479 with 4 species, would have to yield to *Sorbus* on page 477 with 2—necessitating a change of 55 species at the present time. Are American botanists prepared to follow this rule consistently?

Section III is entitled "Compulsory Index for Plant-names." Dr. Kuntze points out that the enormous increase in botanical literature (there are 7000 titles a year at the present time), has made it impossible for any one to go over everything page by page as botanists could do formerly, and that what would have been gross carelessness at one time is almost a necessity now. He therefore proposes for discussion an article to the effect that articles, magazines and works, unless they have an index of names, including synonyms, to each volume, shall not be considered. It is certainly desirable that every work be well indexed. A book without an index, especially in these unsettled times when no one knows where anything will be placed to-morrow, is as good as sealed. But we may well doubt whether the corrective proposed is not too severe. Such penalties are not readily enforceable; and in the future, should a reaction set in against the rule, as usually happens with arbitrary rules of the sort, it would result in no little confusion by reason of the scope given for interference with established nomenclature.

The next two sections deal with some rejections of names made by Pfitzer. One point is of interest. Pfitzer in rejecting Kuntze's name *Sirhookera* takes occasion to make fun of it, a sort of objection to which, it must be confessed, too many of Dr. Kuntze's names are liable. Incidentally he compares it to "*Amtsgerichtsrathschultzia.*" Dr. Kuntze, as usual, comes back at him with a long list of such names coined by others, which must stand without doubt. And he points out in addition that Pfitzer retains a number of names with *du, de, O',* and *Van* prefixes, which are not dissimilar to *Sir* in *Sirhookera.* As far as the *validity* of such names goes, Dr. Kuntze is doubtless quite

right. That they are not to be commended and that we have far too many already without any fresh creations of the same sort, is readily apparent from an inspection of the list which he cites in his justification.

Section VI is devoted to a discussion, apropos of certain changes made by Pfitzer, of the "once a synonym always a synonym" rule. This rule is one which commends itself to all who have had anything to do with nomenclature. In their determination to confer upon some one the honor of a genus dedicated to his memory—a doubtful honor since it has been so frightfully abused—botanists have multiplied homonyms in some cases to an incredible extent. The rule seems to have been "if at first you don't succeed," try again indefinitely till you succeed in making the name stick. In Section 9 of the introduction of his *Revisio Generum,* Dr. Kuntze referred to this practice as an "abiding source of danger to botanical nomenclature." And in the same place he gives a list of 150 personal genus names which have been repeated in this manner, two seven times, two six times, and fourteen five times. One of the most confusing results of this species of synonyms is the condition of oscillation in which it often places a name. A recent case may serve as an example. In his monograph of the *Onagrariae* in the *Pflanzenfamilien,* Dr. Raimann in subdividing the genus *Oenothera,* revived Spach's genus *Kneiffia.* This name is one year older than *Kneiffia* of Fries, so that *K. setigera* Fr. must have a new name. But supposing future monographers should differ with Raimann as to the limitation of *Oenothera* and *Kneiffia* Spach should become a synonym once more, then, according to the ordinary rule, we should have to restore *Kneiffia* Fr., and the new name would serve only to swell the crowded ranks of synonyms. In this way the name of a genus of fungi could be kept in a state of oscillation for an indefinite period, depending all the while on the views held by phanerogamists as to the limitations of a genus of flowering plants. This is a state of affairs which mycologists cannot be expected to tolerate, and can result only in disregard on the part of monographers of the rules which permit such things. Many similar cases might be cited. It is apparent, then, that some rule is necessary by which this difficulty of genus-names in a state of indefinite suspension can be obviated. The plan which at once suggests itself is to invalidate all subsequent homonyms, so that after a name has been once used it cannot be applied to another group. This is done by the "once a synonym, always a synonym" rule.

But Dr. Kuntze, while recognizing the necessity of some such rule, points out that if given retroactive force, the rule in question will involve us in no little difficulty. He gives a list of 200 generic names, all personal names, which must be rejected under the rule, and states that an exhaustive list would include from 500 to 600 generic names and involve about 7000 species. To this formidable number, should be added a large number of species which will be affected by the application of the rule to specific names. Not only is the rule open to this objection, but Dr. Kuntze makes the further point that, like all retrospective legislation, it does great injustice to past workers who knew no such rule. He, therefore, objects strenuously to any retroactive application of it. But, on the other hand, he recognizes the necessity of making provision for cases like the one detailed above, and he has a suggestion which is well worth considering. In his *Codex Emendatus*-(Rev. Gen. III, 1, CCCXIII), he proposes the following addition to Article 60 (I quote from his English text): "Existing homonyms invalidate such homonyms as are in future competitory, or newly established, or renewed." That is, he proposes that the rule be applied to all future cases, and that a name valid now shall not in the future be superseded by any revived homonym. That would obviate the dif ficulty suggested in regard to *Kneiffia* above, and would certainly accomplish all of what is intended by the American rule, without necessitating so many alterations. Dr. Kuntze points out in the present article the impossibility of any permanent nomenclature in large genera without some rule against the revival of homonyms. As an instance he mentions the genus *Panicum*. He says that in working over the species of this genus in his collections "when I found an older name for a species, there were generally also homonyms of other species forthcoming; about which, however, one did not know whether they were valid or not." The only solution of this is a rule which makes a synonym once a synonym for all time. Whether this rule should be made retroactive, or should be applied only to future cases, i. e. to prevent the renewal of existing homonyms and the creation of new ones, is a question which must be decided by those who, from their investigation of the matter, are competent to pass upon it. Dr. Kuntze's suggestion seems to be a wise one and seems to cover all that is required.

The remainder of the article is taken up with the nomenclature of the orchids, and a concluding section relating to a future congress.

Dr. Kuntze has been subjected to a great deal of criticism, some of it unnecessarily severe, though his controversial methods are not always

calculated to placate his opponents. But whatever may be thought of some of his suggestions, we can have little sympathy with those who, as Pfitzer seems inclined to do, charge him with wanton alterations or selfish motives. On the contrary, there is every reason to accept his statement that he was led into the work of reforming nomenclature in the course of the investigation of his collections, a natural thing when dealing with plants collected in every quarter of the globe, which would bring out the defects of our present nomenclature in a most striking manner. After all his work has but served to bring vividly before us what all were dimly conscious of before. Every man for himself was the principal rule of nomenclature in practice. We must at least admire Dr. Kuntze's persistence in endeavoring to bring about uniformity and a better state of things.

ROSCOE POUND.

Notes on the Trees of Northern Nebraska.—These notes apply to the region embraced in Antelope, Holt, Boyd, Rock, Brown, Keya Paha, Cherry, Sheridan, Dawes, and Sioux Counties. In the last three my observations have been much more limited, and, I doubt not, need extension and revision. They are simply good as far as they go.

The country is composed of sandhills interspersed with small lakes, ponds and streams, hay-flats in the moister valleys, and dry valleys between the rows of sandhills, with stretches of dry, firm table-lands, usually abruptly separated from the sandhill portions by a deep cañon stream. With few exceptions, the trees are confined to these cañons, which branch out into the hill-sides in long reaches, some dry, others worn by unfailing spring brooks or "creeks," as they are generally called.

There is good reason to believe that this treeless region was not always thus. On the tops of some of the sandhills have been found decaying trunks of Pine and Red Cedar buried deep in sand, bearing witness to a different condition of moisture in years gone by. In common with most observers, I think, I attribute the change to the destructive prairie fires that have swept over this region from time immemorial. They form one of the chief obstacles, to-day, to the regeneration of the land. The deep cañons are lined, when dry, from summit to base, with *Pinus ponderosa scopulorum* Engelm. A few scattering specimens are found extending several hundred feet upon the neighboring table. When the base of the cañon is wet, the Pine is found only above the line of moisture. It plants its feet in the gray magnesian,

and soft limestone and sandstone rocks, and in the driest season never seems to lack moisture. It belongs to the foothills of the Rocky Mountains, but extends eastward as far as the west line of Holt County in the Niobrara Cañon. The coincidence, at this point, of the Black Walnut (*Juglans nigra* L.) with the Bull Pine is remarkable. In the cañon at Long Pine are many flourishing specimens, young and old, one with the diameter of three feet. The young ones prove that it sometimes fruits, in spite of the late spring frosts. Its western limit is nearly coincident with Brown County and the 100th meridian.

A large block of Black Walnut was found in Cherry County five years ago, not far from Fort Niobrara, and was preserved by Surgeon Wilcox, showing that it once extended further west. This region furnishes but one oak (*Quercus macrocarpa* Michx.), which grows to a large size. It takes the moist and the dry portions of the cañons about equally, where the soil is at all loamy, leaving the most barren parts to the Pine. Its western limit is about the mouth of Snake Creek, Cherry County, about ten miles west of Valentine.

A rare and notable tree is the Canoe Birch (*Betula papyrifera* Marsh), which flourishes only where a dark and sheltered spot is furnished by a steep declivity with a northern exposure. At Fort Niobrara, where these conditions occur in their perfection, surrounded by rare plants such as *Lonicera hirsuta*, *Circaea lutetiana*, *Osmorrhiza claytoni*, *Carex eburnea*, the two latter not having been found elsewhere in Nebraska. You may see noble specimens of this Birch thirty inches in diameter. It is reported sixty miles west and further east on the Niobrara.

The region affords no more useful and hardy tree than the Ash, of which we have two species:—the common species from Antelope County west to Brown is *Fraxinus lanceolata* Borck., from Brown Co. west to the Hills, *Fraxinus pennsylvanica* Marsh. It is not always easy to distinguish them, as Gray gives a pubescent form of the Green Ash. It occupies the same soil as the Oak, running from the water's edge over the cañon line upon the prairie, where it has been fortunate enough to escape destruction from fire. We have no tree more capable of enduring the rigors of drouth, heat and cold. It seldom attains a size of over thirty inches in diameter.

The Basswood (*Tilia americana* L.) is found along the Niobrara in Brown County, and probably further east; apparently reaching its western limit in Cherry County, about four miles west of Valentine. It affects the borders of streams.

I can find but one elm (*Ulmus americana* L.), though *Ulmus fulva* Michx. has been reported from Long Pine Cañon. This elm is one of the best trees for the region, not only flourishing on the water line, but capable of growing on the uplands almost as well, if protected from fire. It attains a diameter of about four feet, and is universal. The Hackberry is found with it (*Celtis occidentalis* L.), but is much less common and only half the size.

The largest tree of the region is the Cottonwood (*Populus monilifera* Ait.), one specimen in Hat Creek Basin, Sioux County, having a diameter of over five feet. This species is common everywhere along streams, and quickly establishes itself in low meadows by means of its tufted seeds, if not destroyed by fire or mowing-machine. In Dawes and Sioux Counties, *Populus angustifolia* James is found in similar situations. One or two others have been reported.

The only tree willow of the region is *Salix amygdaloides* Anders. I long supposed that *Salix nigra* was common throughout the State, but can find no trace of it here. This tree hangs over the streams, reaching a foot or more in diameter. In this connection it is desirable for me to state that since writing on the shrubs of this region (September NATURALIST, p. 803), in which I mentioned a large willow of the *Cordata angustata* variety, at Ewing, Holt County, I measured the "shrub" in question, and found it twenty-eight inches in circumference, and eighteen feet high, several similar trees growing in the one clump from one root. I think we may say that it has reached "tree-like proportions," though retaining the habit of the shrub.

—J. M. BATES.

Valentine, Nebraska.

Messrs. Rand and Redfield on Nomenclature.[3]—A new contribution to the nomenclature problem has recently appeared in the form of a protest against the Rochester Rules in the Introduction to Rand & Redfield's "Flora of Mount Desert." Although the phases of the question there discussed are by this time rather hackneyed, the tone of the article is so confident, and some of its positions are so amazing, that a few remarks thereon may not be amiss.

Had the authors contented themselves with stating that they adopted the nomenclature of Gray's Manual because most of those who would have occasion to use their book would be likely to use it in connection with the Manual, nothing could be said. Such a course has much to be said in its favor. But they have thought best to strengthen their con-

[3]Read before the Botanical Seminar of the University of Nebraska, Nov. 3, 1894.

clusion by an attack upon the Rochester Rules, upon the principles upon which they suppose them to be based, and upon their framers. In the course of this they display a most wonderful ignorance of the whole subject.

In the first place they assume that there was, up to the time the Rochester Rules were framed, a generally received nomenclature, and that the rules in question have overturned it—or have attempted to overturn it. To use their own language, they state that the Rochester Rules are intended to " upset important results of nomenclature evolution for a century and a half." The notion that there has been any fixed or well-defined set of rules " generally followed," or any " generally received " nomenclature, is mostly confined to those whose acqaintance with botanical literature begins and ends with Gray's Manual. To others it has long been apparent that the only generally received. principle was, for the monographers, everyone for himself, and, for the rest of the world, follow the latest monographer. It was to put an end to this, for America at least, and to establish a nomenclature which might have some chance of becoming generally received, and which the next editions of our manuals could not overthrow at the caprice of their authors or editors, that the Rochester Rules were framed.

I have said that the notion that there was a " generally received " nomenclature, was confined mostly to the readers of Gray's Manual. But an examination of that work will speedily show that even the illustrious author of the Manual was far from being sure of " where he was at " in nomenclature.

In the preface to the last edition of the Manual, the editor states that the nomenclature there used conforms to the latest views of Dr. Gray. A comparison with the nomenclature of the preceding editions is, therefore, interesting. One of the first things that one notices is that many changes in the nomenclature of the fifth edition have been made to conform to the " Kew Rule." For instance:

In the fifth edition we find: *Chiogenes hispidula* Torr., *Ilysanthes gratioloides* Benth., *Xerophyllum asphodeloides* Nutt., *Bouteloua curtipendula* Gray. These specific names represent in each case the oldest name: *Vaccinium hispidulum* L., *Capraria gratioloides* L., *Helonias asphodeloides* L., *Chloris curtipendula* Michx. In the sixth edition these appear as *Chiogenes serpyllifolia* Salisb., *Ilysanthes riparia* Raf., *Xerophyllum setifolium* Michx., *Bouteloua racemosa* Lag., the names allowable under the Kew Rule. In the 1848 edition also, we find *Bouteloua racemosa*. That is, in 1848, Dr. Gray followed the Kew Rule in this particular instance, while disregarding it in the other cases mentioned.

In 1868, he thought otherwise as to this one name and used the oldest specific name, while adhering to the Kew Rule in many cases (e. g., *Lophantus anisatus* Benth. = *Hyssopus anisatus* Nutt., 1818, = *Stachys foenicula* Pursh, 1814). In 1889, his editor, representing "his known and expressed views," changed about as to all of the names in the list just given, and altered a large number of names to conform to the Kew Rule, still, however, disregarding it in some cases. At the same time the editor stated that "reasonable regard" had been had to the claims of priority! This last promise was fulfilled by changing about a dozen specific names and two or three generic names so as to use prior names. For instance, in the fifth edition we find *Nelumbium* Juss. In the last edition, *Nelumbo* Tourn. The name which the Rochester Rules would require is *Nelumbo* Adans. If the editor was willing to alter the name to which Dr. Gray had given currency for thirty years, and to go back to Tournefort for a name, others can scarcely be blamed for following his example in similar cases, and going back at least to the time of Linné. A long list might be made showing the wholly arbitrary and personal character of the alterations made in the nomenclature of the successive editions of the Manual. It is needless, however, since the facts are generally known. No reproach is implied in this so far as the illustrious author of the Manual is concerned, for he only did as all others were doing—namely, followed his personal inclination at the moment in each specific case. But such a condition was a reproach to botanical nomenclature, and could only result in a revolution.

While American botany was in its infancy, it was natural that all should follow blindly in the wake of one great man. It is no less natural that the botanists of to-day should demand something more than a great name to justify uncertainty and vacillation in nomenclature. It is, in reality, the so-called conservatives who stand for disorder and confusion in nomenclature. *They* are the "Rip Van Winkles just awakened from a comfortable nap of years," and somewhat rudely awakened, too, thanks to Dr. Kuntze, and not over-clear in consequence as to who or where they are.

It takes but a moment's glance at the successive editions of the Manual to show how utterly baseless is the notion that the framers of the Rochester Rules are seeking to overthrow "well-established principles of property rights, custom, usage, and the well-established maxim, *quieta non movere*." The greater part of the rules adopted at Rochester were rules which botanists had, for many years, at least professed to recognize. The fact that the only representative body of American botanists was compelled to legislate on the subject shows of itself that

the state of nomenclature was far from quiet. Anyone who thinks that all was peaceable and serene till Dr. Kuntze and the Rochester Rules came down upon the fold, should be somewhat cautious in his references to Rip Van Winkle. When the most conservative of authors fails to reveal any system or principle consistently followed out in the several editions of his widest known work, and when contemporary works are in hopeless disagreement with themselves and with the Manual, it sounds somewhat strangely to be told that we are cutting "the solid ground from beneath our feet" in laying down a set of rules and principles and agreeing to abide by them. When everybody made changes in nomenclature to suit his personal fancy, no one made any remonstrance, and we all followed the changes of the latest monographer without hesitation. It is only since this state of affairs has become intolerable to the majority of American botanists, and they have resolved to make changes in nomenclature according to rule and principle, and not according to personal taste and caprice, that any complaint has been heard.

The authors also protest against the representative character of the members of the Rochester and Madison meetings, and refer to them as "comparatively few botanists of various degrees of repute." Whether this means that Boston still thinks herself the centre and focus of American learning in all branches, and that the authors regard all of those poor mortals who do not live in the shadow of Cambridge as intruders, or whether it is only another instance of Rip Van Winkle, one need not enquire. The remarks of the authors remind one of some editorial sayings in Zoe *apropos* of the Madison Congress and of the American Botanical Society. The botanists who dissent from the principles of the Rochester Rules certainly have not made much "noise," and the world at large is likely to be glad to know who they are. It will also be glad to know who those botanists are who possess "that added grasp of affairs" which, we are told, in addition to mere knowledge of herbaria and of the literature of the subject, is necessary to qualify a botanist and make him competent to pass on questions of nomenclature. The statements as to the *personnel* of the Rochester meeting fall little short of impertinence.—Roscoe Pound.

Botanical News.—The University of Chicago announces botanical lectures and laboratory work by Dr. John M. Coulter, who is styled the Professorial Lecturer on Botany. This would seem to indicate that eventually this great University may call Dr. Coulter to build up a department of botany commensurate with its importance.

In September the National Herbarium in Washington was transferred from the Agricultural Building to fire-proof rooms in the eastern pavilion of the National Museum. It will still be under the control and care of Chief Botanist Coville and his corps of assistants.

Parts I and II of the "Flora of Nebraska" by the Botanical Seminae of the University of Nebraska have been published. They aggregate seventy-eight pages of descriptive text and thirty-six plates, and include the Schizophyceæ, Chlorophyceæ, Coleochætaceæ, Rhodophyceæ and Charophyceæ.

The Proceedings of the Madison Botanical Congress have been issued by the Secretary, Dr. J. C. Arthur, of Lafayette, Indiana, in a neatly printed pamphlet of sixty pages.

ZOOLOGY.

Terminology of the Nerve Cell.—Fish attempts[1] to avoid some of the confused terminology of Neurology by proposing a consistent nomenclature, adopting to some extent existing terms. Thus he would call the entire nerve cell, with its appendages, neurocyte; the axis cylinder prolongation; neurite; the other processes dendrites, and the neuroglia cell, spongiocyte. Nerve cells would then be dendritic or adendritic, mono-or dineuritic, etc., according to the number and character of the processes concerned.

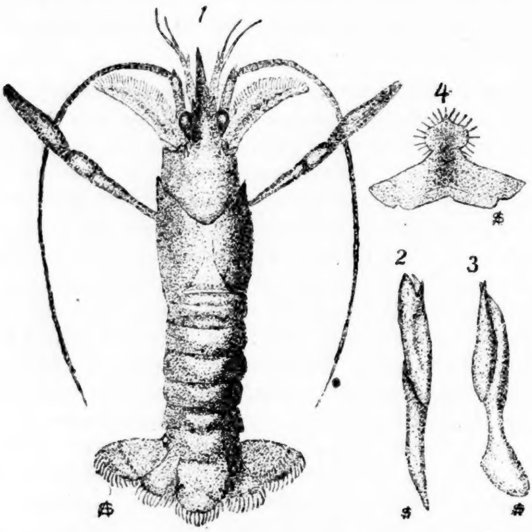
Structure of Clepsine.—Oka has attempted[2] the solution of some of the problems of Hirudinean anatomy. After some remarks on external morphology, he takes up in succession the body cavity, blood vessels, nephridia and the systematic position of these animals. The text is rendered much more easy of comprehension from the reconstructions on the plates. Oka recognizes in the lacunæ of the body the true cœlom which is broken up into a large number of anastomosing cavities, in which may be recognized the following principal regions: in the middle of the body, a median dorsal and a median ventral lacuna, in each of which run blood vessels. In front and behind these fuse into a "median" lacuna. These lacunæ are connected by short canals with a complicated "zwischenlacuna," which runs the length of the body on either side, and this in turn by segmentally arranged tubes with a lateral lacuna on either side. These various spaces are also connected with a subepidermal system of lacunæ, the principal canals of which correspond to the annuli of the external surface. In the blood vascular system, which is cut off completely from the lacunar cavities, segmentation has largely disappeared. In but few regions can even the most remote resemblance to a segmental arrangement of vessels be traced, although the dorsal vessel shows segmental enlargements. The nephridia are described at length, the account confirming and supplementing the descriptions of Whitman, Bourne and others, and disagreeing *in toto* with those of Bolsius, except in that they confirm the latter in the description of an ectodermal terminal portion.

In conclusion, Oka thinks the Hirudinei nearest the Oligochætes, basing this view upon chiefly three factors: (1) the existence of a seg-

[1] Jour. Comp. Neurology, iv, 1894.
[2] Zeitschr. wiss. Zool., lviii, 1894.

mented cœlom; (2) a blood vascular system distinct from the cœlom and (3) a pair of nephridia in each somite; points which it seems to the present writer, imply only Annelid affinities since they fit Polychætes as well as Oligochætes.

A new Cambarus from Arkansas.—*Cambarus faxonii* sp. nov. Male, form 1, rostrum broad, elongate, deeply excavated above, margins raised into sharp parallel ridges, each ending in prominent spines Acumen very long and slender, curved upwards; post orbital ridges prominent, each ending in a prominent spine.



Carapax cylindrical, slightly compressed, smooth; cervical groove moderate, a prominent spine on each side. Distance from cervical groove to posterior margin of carapax 2½ to 3 in distance from cervical group to tip of acumen, and equal to length of acumen. Anterior 1–2 of the areola narrow, its posterior portion triangular. Abdomen broad and slightly shorter than cephalothorax (including acumen). Outer posterior part of telson ending in a prominent spine inside of which is a much smaller spine, posterior margin of telson slightly emarginate. Anterior process of epistoma triangular. Basal segments of antennules with a spine on under inner border, about middle of segment. Antennæ shorter than the body, antennal scale long and narrow (i

length almost three times its greatest width), slightly curved outward and ending in a sharp spine, equals the rostrum.

Basal segment of antennal scale with a prominent spines on anterior lateral borders. Chelipeds slender, not tuberculated, slightly hairy; fingers shorter than hand, opposed margins of the fingers straight, hand smooth; carpus smooth; a spine on inner and outer distal borders. Meros smooth with one spine on upper and one on outer side, and two below, all spines on distal 1–3. Third pair of legs hooked, fifth pair with a small roundish tubercle on basal joint.

Anterior abdominal appendages strong and of moderate length, tips reaching between third pairs of legs, bifid at apex, apex of inner part posterior and acute, its tip turned slightly outward, outer bluntish.

Color of this species somewhat mottled with bluish on antennal scale and rostrum, forming cross bars.

This is apparently a small species. The largest specimens taken were females, length (from tip of acumen to posterior margin of telson) of largest specimens, 2½ inches. The size of average males, 2¼ inches.

This species is easily recognized by its long, slender acumen, small hand, slender antennal scale and its small size. Found in St. Francis River at Greenway and Big Bay. It is by no means abundant. This and young of one other species, *C. palmeri*, are all I found in the St. Francis River.

Named in honor of Dr. Walter Facon, to whom we owe more than to anyone else our knowledge of North American crayfishes.

Explanation of Figures.

1. Dorsal view of specimen, x, 1.31.
2. Abdominal appendage, inner view, x, 4.35.
3. Abdominal appendage, posterior view, x, 4.35.
4. Epistoma, x, 4.

The drawings were made by Miss Allie Simonds, Arkansas University, Class 1895.

S. E. Meek,
Arkansas University,

Oct. 22, 1894, Fayetteville, Ark.

A New Bassalian Type of Crabs.—In a recent number of the Journal of the Asiatic Society of Bengal (v. 63, part 2, No. 3), a most remarkable crab has been described and illustrated by Messrs. A. Alcock and A. R. Anderson. It has been designated (p. 141) as "*Archæ-*

oplax, a Gonoplacid (?) crab of a remarkably antique facies, which appears to be closely connected also with *Cymopolia*."[3]

The description and figures appear to me to indicate that the new crab has no close relationships with either the Gonoplacids or *Cymopolia*.

Through the kindness of Miss Rathbun, of the Invertebrate department of the U. S. National Museum, I have been able to study specimens of all types and compared them with the data respecting *Archæoplax*, and could find no special features of agreement. *Archæoplax*, it seems to me, must be considered entirely independently of the types with which it has been contrasted.

I may preface the further remarks I have to make with the statement that the crab so called by Messrs. Alcock and Anderson cannot retain the name given to it by them—*Archæoplax*—as precisely the same form had been bestowed more than 30 years ago on an extinct genus, also of the superfamily of Grapsoidea, represented by fossils from Gay Head, Mass. *Archæoplax signifera* was the name given by W. Stimpson to miocene tertiary remains found there, and described in the Boston Journal of Nat. Hist. (vol. 7, p. 584, 1863).

As a new name is therefore necessary, I would suggest as eminently appropriate for the crab made known by Messrs. Alcock and Anderson, the generic designation *Retropluma* (*retro*, back or backward, and *pluma*, a soft feather). The applicability will become evident in due course.

When I first saw the figure of the mouth parts I inferred that the external pair of maxillipeds had been lost, but Messrs. Alcock and Anderson expressly declare (p. 182) that "the external maxillipeds are so small and slender as to leave completely exposed the mandibles, the wide endostome, and a part of the wide and produced efferent branchial channels." They give the figures as those of a perfect animal, and apparently had a number of specimens.[4] We are, therefore, placed in the dilemma of assuming that the crab differs radically from all others, or that the learned authors may have been mistaken; I prefer, in this dilemma, to leave the question open for re-examination by the original describers.

The new type, however, differs in another character almost as remarkable as would be such an extreme and anomalous modification of the maxillipeds supposed by its describers.

[3] It is later (p. 180) suggested that "its nearer affinities are, perhaps, with the Macrophthalmines."

[4] "Bay of Bengal, at almost all stations off the Coromandel coast, from 140 southwards, between 100 and 250 fms." P. 183.

"The fifth pair of trunk legs is quite unique in form and disposition: they arise quite close to the middle line of the body and high up, almost on the back; they are short, being considerably less than the breadth of the carapace in length, and are very slender and flexible; and they are so thickly fringed with shaggy hairs as to appear like feathers."

This peculiar modification of the last pair of limbs is very unlike that of the corresponding legs in the notopodous or anomurous brachyurans, and indicates that some special function may be assumed. The loss of geniculation and the straightness, the slenderness and flexibility, and the dense hairylike covering must mean something. May it not be that the peculiarly modified limbs have been specialized for purposes of aërification of an increased vascular supply, and that they have become functionalized as branchiæ? Until some better hypothesis can be suggested or tested by histological examination, bold as it may seem, the explanation cannot be considered irrational.

As has been already remarked, *Retropluma* has no close relationship with the forms compared with it or with any other known types. It should, therefore, be regarded as the representative of an independent family—*Retroplumidæ*—especially characterized by the peculiarly modified fifth pair of feet, want of true orbits, and position of the antennæ. For the present it may be retained in the superfamily or tribe *Grapsoidea*, on account of the reduced number of branchiæ ("six on each side") and form of body. If, however, the illustrations and description of the mouth parts are correct, it must be widely removed. The only known species is *Retropluma notopus*.

I cannot appreciate any "remarkably antique facies in the new crab." On the contrary, it appears to be a form excessively modified for deep sea life.—THEO. GILL.

Note on the Occurrence of Hyla andersonii in New Jersey.—About the middle of June, 1889, Mr. Louis M. Glackens and the writer were engaged in general biological studies along the Atsion and Batsto Creeks, in Atlantic and Burlington Counties, New Jersey. On the night of June 17th we stopped at Pleasant Mills. Shortly before sundown a thunder storm arose, just previous to and during which the frogs became very noisy in a swampy thicket near by.

The note was an unfamiliar one and invited investigation, which resulted in the capture of two specimens of this handsome and rare species. The shrill quack-ack, which at the time was compared to the note of a frightened guinea fowl, and which is not unlike the call of a

rail, was constant and seemed to come from every tree; but during our progress through the thicket the voices immediately around us, for a radius of about 25 feet, were silent. This circumstance and the oncoming darkness made it difficult to secure specimens, although the frogs were so abundant. The two secured were found perched on the lower sides of branches of the pines with dilated and vibrating throats, though at the moment they were silent; and it was noted that they emitted an odor which was likened to that of raw green peas. The color above in life was a bright pea green, quite unlike the dull olive green of spirit-preserved specimens. The lateral stripe was of a very rich velvety purple. The following morning we could find no trace of them, but later in the day heard another chorus in the middle of a dense swampy thicket. Since then Mr. H. F. Moore and myself have repeatedly visited the locality in quest of the Hyla and its eggs, but entirely without success. To the natives the frog is unknown.—J. Percy Moore.

Yolk Nucleus of Cymatagaster.—J. W. Hubbard, in a paper,[5] the proof-reading of which could be better, shows that the yolk nucleus in these fish eggs is produced from the true nucleus, soon after the cell becomes differentiated as an egg, that it migrates towards the vegetative pole, and after the closure of the blastopore, it breaks up and disappears in the yolk. He claims that the same structure occurs in many eggs and has been mistaken for the spermatozoon, and thinks it homologous with the meganucleus of the Protozoa, a conclusion which needs more support than is advanced in the paper. The review of the literature omits several important papers.

Zoological News. Protozoa.—Gruber, in his Amöben-Studien,[6] comments on the great rarity of observations on the division of the Amœba, and especially calls attention to the absence of any observations upon the mitotic division of the nucleus. He calls upon other observers to make observations on this point. He has had an opportunity of directly comparing Rhizopods from Massachusetts and from the Black Forest, and says that the forms from the two localities are identical. Some remarks are made upon specific characters in the Rhizopods.

Cœlenterata.—Grieg, in a paper but recently received,[7] catalogues 30 species of Pennatulida as belonging to the Norwegian fauna.

[5] Proc. Am. Philos. Soc., xxxiii, 1894.
[6] Bericht Naturf. Gesellsch., Freiburg, viii, 1894.
[7] Bergens Museums Aarsberetung for 1891, 1892.

Apellöf, in the same volume, describes several structures in the anatomy of Edwardsia. Among the points brought out are the presence of a nervous system in the capitulum, the absence of siphonoghyphes, of septal stomata, of acontia. Its nearest affinities appear to be with Protanthea of Carlgren (1891).

WORMS.—Stiles calls attention [8] to the discovery in a cat, by H. B. Ward, of *Distoma westermanni*, a fluke new to the U. S. The same species is a common parasite in man in Eastern Asia.

Ward describes [9] *Distoma opacum*, parasitic in *Amia calva*, *Ictalurus punctatus*, and *Perca flavescens*. In its structural characters the species is closest to *D. pygmæum* of the eider duck. The fish become infested by feeding upon crayfish (*Cambarus propinquus*), in which the parasite was found encysted.

CRUSTACEA.—Miss Mary J. Rathbun describes[10] four new species of crabs from the Antillean region and gives[11] a series of notes upon the species of Inachidæ in the National Museum. There seems to be a tendency in these and other papers to differentiate genera and species on too minute and too variable characters, which, we hope, will not be continued in the promised Synopsis of North American Crustacea.

ARACHINDA.—Purcell's complete paper on the eyes of harvestmen has appeared,[12] and the illustrations make clear the difficulties of his previous paper, already noticed (this volume, p. 345).

Bernard[13] calls attention to the fact that the Galeodidæ, instead of lacking lateral eyes, have these organs transferred to the lateral surface, where they look downwards and forwards. Bernard thinks these organs are in process of atrophy, although one would not draw such conclusions from the rough figure of a section which he gives.

Simmons describes [14] the development of the lungs and tracheæ in spiders. The lungs develop on the posterior surface of the anterior abdominal appendages, and the appendages, sinking in form the anterior wall of the pulmonary sac. The tracheæ in their earlier stages are like the lungs, and later begin to penetrate the body. "From this it follows that the lung-book condition is the primitive one, the

[8] Johns Hopkins Hospital Bulletin, No. 40, 1894.

[9] Proc. Am. Soc. Microscopists, xv, 1894.

[10] Proc. U. S. Nat. Mus., xvii, p. 83, 1894.

[11] Tom. Cit., p. 43.

[12] Zeitschr. Wiss. Zool., lviii, 1894.

[13] Ann. and Mag. Nat. Hist, xiii, 517, 1894.

[14] Am. Jour. Sci., xlviii, 1894. Tuft's College Studies, No. 2.

tracheæ of the Arachnids being derived from it. And with these facts these is left no ground for those who regard the 'Tracheata' as a natural group of the animal kingdom."

HEXAPODA.—Schott has a monograph of palæarctic Thysanures in Vol. xxv of the Handlingar of the Swedish Academy, 133 species and varieties are enumerated, of which 9 are new. Seven plates illustrate the article, which cannot be neglected by entomologists.

A most interesting paper on the relations between attitude and color of European butterflies is given by Dr. Standfuss in the Zürich Society's Vierteljahrschrift for 1894.

HEXAPODA.—Scudder gives[15] a synopsis of the ringless locustarians of the tribe Ceuthophili. Six genera and 67 species are described.

MOLLUSCA.—Dall has monographed[16] the genus Gnathodon. From a consideration of large suites of specimens, and of young as well as old, and also from a study of the soft parts, he concludes that the genus is distinctly Mactroid in character. Ten species and varieties are enumerated.

Dr. Stearns[17] catalogues, with notes, a collection of shells from Lower California and adjacent waters, made by W. J. Fisher in 1876, together with those of other collectors. The paper has great value in matters of synonymy and geographical limits of species.

Apellöff records[18] the presence of several North American species of Cephalopods on the Norwegian coasts, and describes an example of *Eledone cirrhosa* in which the third right arm of both sides is hecotocotylized.

FISHES.—Gill shows[19] that our American pike perches must continue to bear the generic name Stizostedion, and that the European *Lucioperca marina* has more affinities with the other European species than with any American forms.

The same author also pleads[20] for the use of Pœciliidæ instead of Cyprinodontidæ, and discusses the nomenclature of the Lampreys, discarding his previously advanced name of Ammocœtes for the genus Lampetra. He further makes a family Mordaciidæ for the genus

[15] Proc. Amer. Acad. Arts and Sciences. xxx, 1894.
[16] Proc. U. S. Nat. Mus., xvii, 1894.
[17] Tom. Cit., 1894.
[18] Bergens Museums Aarbog for 1892, 1893.
[19] Proc. U. S. Nat. Mus., xvii, 1894.
[20] L. c.

Mordacia. In a fourth paper he discusses the subdivisions and relationships of the Salmonidæ and Thymallidæ.

E. D. Cope catalogues[21] a collection of 42 Fishes from the Rio Grande do Sul, Brazil. Of these, 17 are new. The species of Characinidæ and Siluridæ, 15 and 14 respectively, predominate.

Batrachia.—Miss Platt has published[22] her complete paper on the origin of the cartilaginous structures in the head of Nicturus, to which reference was made on p. 637 of the present volume.

Peter has studied[23] the vertebræ of the Cæcilians, and concludes that the evidence from these structures justifies the view of Wiedersheim (1879) and Cope (1884) that these forms should be assigned to Urodela. Regarding Cope's view, adopted by the Sarasins, that in Amphiuma we must recognize the ancestral form of the Cæcilians, Peter says, "there is indeed a certain similarity in the vertebræ of Apoda and Amphiumidæ, but no greater than exists between them and Siren, so that the view of this student is supported chiefly by developmental conditions."

Mammals.—Dr. E. A. Mearns describes[24] as new, *Sigmodon minima*, from New Nexico.

Dr. J. A. Allen points out[25] that the skull in Neotoma is extremely variable, and that "species" founded on certain cranial characters are frequently not of varietal rank.

[21] Proc. Am. Philos. Soc., xxxiii, 1894.

[22] Archiv für mikros. Anat., xliii, p. 911, 1894.

[23] Karl Peter. Die Wirbelsäule der Gymnophionen. Dissertation. Freiburg, 1894.

[24] Proc. U. S. Nat. Mus., xvii, 1894.

[25] Bulletin Amer. Mus. Nat. Hist., vi, 1894.

ENTOMOLOGY.[1]

Some Observations on the Distribution of Coccidæ.[2]—Being now in the midst of preparing a new list of the known Coccidæ, with notes as to food-plants, distribution, etc., I have thought it opportune to submit to you a few observations which seem to me to be of interest, relating to the geographical distribution of the several genera. In preparing these notes, I have, moreover, been moved by a lively hope that some of you who have so much unpublished information regarding this group of insects, may be induced to throw a little fresh light on points which are now obscure. More especially do I refer to the numerous undescribed species which must doubtless exist in the collections at Washington, information of which would so greatly help to fill up blanks now too apparent to those who read our lists with a critical eye.

The following genera, some of them not very well established, are monotypic according to present information.

Walkeriana Sign.; Ceylon.
Guerinia Sign.; Mediterranean Region.
Tessarobelus Montr.; New Caledonia.
Drosicha Walk.; Ceylon and China.
Llaveia Sign.; Mexico.
Nidularia Targ.; Europe.
Capulinia Sign.; Mexico.
Cerococcus Comst.; Arizona, California.
Xylococcus Löw; Austria.
Callipappus Guèr.; Australia.
Rhizœcus Künck., in hort (from Australia?).
Puto Sign.; Europe.
Tetrura Licht.; Europe.
Cryptococcus Dougl.; Europe.
Signoretia Targ.; Europe and Australia.
Fillippia Targ.; Europe.
Pseudopulvinaria Atkins.; Sikkim.
Vinsonia Sign.; West Indies, etc.
Physokermes Targ.; Europe.
Aclerda Sign.; France.

[1] Edited by Clarence M. Weed, New Hampshire College, Durham, N. H.
[2] Read before the Entomological Society of Washington, Oct. 11, 1894.

Spermococcus Giard.; France.

Exærctopus Newst.; Channel Is.

Ericerus Guèr.; China.

Fairmairia Sign.; France.

Ischnaspis Dougl.; West Indies, etc.

Frenchia Mask.; Australia.

Of the above twenty-six monotypic genera, most of which are undoubtedly valid (seven, perhaps, might be questioned), it will be seen that just half are European, four are Oriental, four appear to belong to the Australian region, two are Mexican, two are marked as from the West Indies, etc., and one is from the arid portion of the United States.

Signoretia offers a singular case, the European species being represented in Australia by a form which Maskell separates from it only as a variety. Supposed endemic species of *Signoretia* from Australia and New Mexico prove to belong to *Pulvinaria* and *Bergrothia* respectively; and it is difficult to avoid the conclusion that *S. luzulæ* var. *australis* Maskell, from Australia, must be *S. luzulæ* which has been introduced and has varied from the type under its new environment. If so, the matter deserves the close attention of evolutionists.

It is curious that the common *Physokermes* of Europe has no representative here in America. We have two species of *Lecanium* on conifers, one in Canada, the other in California, but they are not like *Physokermes.*

So, also, we seem to have no representative of the subterranean European genera, *Aclerda, Spermococcus* and *Exærctopus.* Do our ants' nests never harbor such?

Fairmairia has a close ally in northern Mexico and New Mexico in *Ceroplastodes*—the latter with two species. A curiously similar case is offered by *Lichtensia,* which has one species in Europe and another in Vera Cruz, Mexico. The latter, one of the most beautiful of Coccidæ, from its brilliant yellow color, cannot be made the type of a distinct genus, though it is very different from its European congener.

Vinsonia and *Ischnaspis* (the latter near to *Fiorinia*) are common on cultivated plants in the West Indies, but the specimens offer no chance for the separation of even varieties. *Ischnaspis,* it will be noted, is the only monotypic genus of Diaspinæ.

The Monophlebinæ appear to be ancient forms, probably at one time more abundant than now. They have been found fossil both in Europe and America; and the existing genera are represented by comparatively few species widely scattered over the earth, after the

manner of *Peripatus*. Thus, *Palæococcus*, to which the fossil species are assigned, has three living species, one in Europe, one in South America and one in New Zealand.

Ortonia has also three species; one from Natal, the other two neotropical.

Icerya appears to be neotropical, Oriental and Australian; and there is an allied genus or subgenus, which I hope Prof. Riley will soon describe, found here in New Mexico.

Porphyrophora is considered Palæarctic, but has its representative in America in *Margarodes*, with one West Indian and one Chilian species. *Cœlostoma* is confined to Australia and New Zealand, and thus forms an exception among the polytypic monophlebid genera; but *Monophlebus* is recorded from widely separated countries in the Eastern Hemisphere.

Gossyparia has five species, two Palæarctic, two Australian and one from New Zealand—truly a curious distribution!

Eriococcus is interesting. Six species are Palæarctic; Australia and New Zealand together have no less than sixteen, only one of which is common to both these countries, and then the Australian form is a distinct variety of a New Zealand species. No other species whatever are known except three from North America, two of which, *E. azaleæ* and *E. coccineus*, cannot well be native there. In the West Indies, where *Dactylopius* abounds, no *Eriococcus* has been ever seen.

Rhizococcus presents one Palæarctic species, three from Australia and six from New Zealand. We seem to have in this country two undescribed species, however.

Bergrothia, which is very near to *Dactylopius*, has one Palæarctic species; while two very nearly allied forms are found in New Mexico, and referred by me to the same genus. Still another is reported from Indiana, etc., but is undescribed.

Dactylopius seems to be rich in species in most parts of the world, but becomes rare and is supplanted by *Phenacoccus* in the northern parts of the Palæarctic region, such as England. The neotropical species are numerous, but the nearctic forms are singularly few, and (excepting introduced ones) all western. Mr. Coquillett has described them, and I have sent the description of a fourth to the printer. There are nine known species from Australia and eight from New Zealand; for the most part these differ in type from the neotropical forms, so that it might be proposed to place them in a distinct subgenus. The genus *Dactylopius*, as now understood, contains very divergent forms, but great difficulty is felt in any attempt to separate it into subgeneric groups.

Phenacoccus is rich in Palæarctic species, there being eleven or twelve, several recently (1886–1891) described. In strong contrast, we have but two endemic nearctic species, both western. There is not one from the neotropical region, but Australia furnishes two and New Zealand one.

Ripersia has five Palæarctic species, three from New Zealand and one from Australia. It was thought that we had none in America, but Mr. N. Banks has discovered a most remarkable maritime species, the description of which now awaits publication. It is very closely allied to one (*R. rumicis*) from New Zealand.

Coccus has three races, perhaps not very distinct as species, from the warmer parts of North America, extending northward in the Rocky Mountain Region. *C. agavium* may be referred to a distinct genus, *Gymnococcus* of Douglas, which should be added to the list of monotypic genera above. Its native country is unknown.

Kermes has several Palæarctic species; one Ethiopian, not yet described; one Australian; and a problematical number nearctic. In the last mentioned region only a single species has been described, but others exist and sorely need attention. No species are neotropical.

Orthezia is doubtless an old form, and certainly a very interesting one. The number of Palæarctic species is a matter of dispute, but there are not over half a dozen. Four are nearctic; and here it may be mentioned that Prof. C. H. T. Townsend has just discovered a beautiful new one in Sonora. Two are neotropical, both described by Douglas. None were known from the Oriental region, until the other day Buckton described one from Ceylon. Not one occurs in Australia or New Zealand.

Prosopophora was described as lately as 1892, but already we know four species, one neotropical, one nearctic (New Mexico), and two from Australia.

Tachardia has four American species, one still awaiting publication. There is, also, one from the Oriental region, while three are Australian.

Pulvinaria is rich in Palæarctic species, but the endemic nearctic species are only three or four! Four are neotropical; two (one undescribed) Oriental; four Australian; and one is from the Sandwich Is. The absence of native species in New Zealand is noteworthy.

Ctenochiton, with eleven species, and *Lecanochiton*, with two, are strictly confined to New Zealand; and may be set off against the numerous extraordinary gall-making forms of Australia, which are wanting in the New Zealand fauna.

Inglisia has five New Zealand species, and until last year was supposed to be confined to that island. But in 1893 Mr. Maskell described one from Australia, while this year I have described a species from Trinidad in the neotropical region.

Ceroplastes has its metropolis in the neotropical region, with thirteen supposed species, some of the most doubtful validity. One only is native in the nearctic region, and that to the south (New Mexico and Northern Mexico), as *C. rusci* is in Europe. One is Ethiopian, two Australian, and two Oriental. Of the last mentioned, *C. ceriferus*, which produces the Indian White Wax, appears to be also widely distributed in the neotropical region. Can it be a survival in both regions, like the tapir—though not, like that, differentiated into species?

Lecanium presents nearly 90 species, several of which, however, may not be valid. The *Eulecanium* series is abundant and widely distributed in the Palæarctic and nearctic regions, but I do not know a single *Eulecanium* from elsewhere. In the tropics the *Bernardia* section, with few but very destructive species, takes its place. The neotropical species, when we eliminate those introduced from elsewhere, amount to only eight, only one of which (*begoniæ*) is a *Bernardia*, and the endemic character of that is a matter for serious doubt. But who shall say that *L. oleæ* and *hemisphericum*, which belong to *Bernardia*, are not neotropical, since they are now so widely spread that their native country cannot be learned? The Oriental species, so far as endemic, are but six, while three peculiar forms are recognized as endemic in Australia. In New Zealand, Mr. Maskell has found but one new species, and that is extremely near to *L. oleæ*.

The above notes will suffice for the purpose intended, though many genera, including the Diaspinæ, are passed over. Defective as our knowledge is, we seem to see some glimmering of light, which should spur us on to further discoveries which will give a sound foundation to our knowledge of Coccid distribution.—T. D. A. COCKERELL, New Mex. Agr. Exper. Station.

Securing Moth's Eggs.—J. B. Lembert describes[3] the following method of securing eggs of moths: " When I take an *Arctia ornata* ♀ and she is ready to lay eggs, the moment she shows signs of being stupefied in the cyanide bottle, I take her out, close the wings over her back, and place her in a paper envelope; as soon as she revives she will commence to scratch the paper with her legs; I then shake the envelope, and if she has given up some eggs, I take them out, give her

[3] Can. Entomologist, June, 1894.

another dose of cyanide fumes, and when she revives a second time I have found as many as 125 eggs in the paper." The method has also been successfully used in securing the eggs of butterflies.

American Species of Seira.—In a paper on the American species of the Thysanouran genus Seira[4] Prof. F. L. Harvey describes *S. mimica* n. sp., which resembles *S. nigromaculata* Lubbock, but differs in the color and the arrangement of the color patches. It is found in warm, dry situations about buildings. *S. bulkii* Lubbock was also found at Orono, Me., under conditions which indicated that it was indigenous.

Kentucky Orthoptera.—Prof. H. Garman publishes, in the Sixth Annual Report of the Kentucky Agricultural Experiment Station, a valuable list of the Orthoptera of that State. In introductory paragraphs he makes the following remarks which are of general biological interest:

"The fauna of the State presents no well-marked features of its own. The eastern half of the State evidently forms part of an eastern zoological region, while the western half is as evidently southern in general character. The species occurring within our limits fall under five categories, as follows: (1) Those which occur everywhere in the United States, such as *Gryllus abbreviatus, Hippiscus rugosus, Chortophaga viridifasciata, Pezotettix bivittatus, P. femurrubrum* and *P. atlanis.* (2) Those which belong to the eastern region, represented by *Acridium alutaceum, A. rubiginosum* and *Paroxya atlantica.* (3) Southern species, such as *Schistocerca americana, Anisomorpha buprestoides* and *Stagmomantis carolina.* (4) Western species, such as *Pezotettix differentialis* and *Mestobregma cincta.* (5) Cave species, of which we have three.

"In Eastern Kentucky the fauna is, as a whole, eastern and northern in character, rather than southern, probably because of the greater elevation above sea level of this part of the State. The southern species show a marked increase in abundance in this section as one approaches the southern boundary of the State. Here the northern limit of the Austroriparian region may be said to coincide with the boundary between Kentucky and Tennessee, and so continues to the headwaters of the Barren River, where a sharp northward extension occurs, bearing gradually northwestward, and following along the eastern limits of our western coal fields to enter southern Indiana and

[4] *Psyche*, Nov., 1894.

Illinois. I could not perceive any very decided southern features of fauna or flora at Campbellsville and Greensburg, near the headwaters of Green River. At Bowling Green and Glasgow Junction the southern character is decided. At Elizabethtown, farther north and east, the fauna and flora do not appear to be very different in relative abundance of species from those of the region about Lexington. The eastern limit of the northward extension of the Austroriparian region would thus appear to follow approximately the meridian marking the 86th degree of longitude west from Greenwich, and accompanies a fall in altitude to about 500 feet above sea level, the blue-grass region to the eastward being in the neighborhood of five hundred feet higher than the region west of Leitchfield. This western region is marked not only by an increased abundance of southern Orthoptera, but quite as decidedly by its other insects, its plants, and its vertebrate animals. Among Lepidoptera, *Callidryas eubule* and *Euthisanotia tamais* become noticeable. The water moccasin (*Ancistrodon piscivorus*) and the shining bass (*Centrarchus macropterus*) appear. There is a decided increase in the numbers of such birds as the tufted titmouse, summer redbird and scarlet tanager.

"We find here the spider-lily (*Hymenocallis occidentalis*), the American aloe (*Agave virginica*), the willow oak (*Quercus phellos*), the water-locust (*Gleditschia aquatica*) and the Mississippi hackberry (*Celtis mississippiensis*).

"Among the Orthoptera found in this end of the State two are worthy of special mention because their occurrence is in some respects exceptional. *Mestobregma cincta* is recorded by collectors from Colorado and Wyoming. Dr. Cyrus Thomas obtained examples from Southern Illinois. I recently collected specimens at Glasgow Junction and Bowling Green in this State. I have no record at hand relating to its occurrence in regions between these widely separated eastern and western habitats. The second species is *Pezotettix differentialis*, the

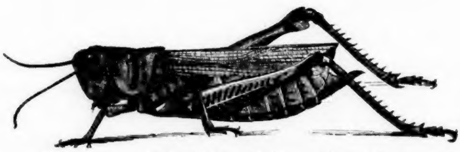


Fig. 1. *Pezotettix differentialis.* After Riley.

large olive grasshopper so common in the northwest. It appears to be one of a relatively small number of northern species whose distribu-

tion is extended to the southward by the influence of the Mississippi River. The species is one of the commonest Illinois grasshoppers. It is common locally in Western Kentucky, but has not been seen eastward.

"The peculiar cave Orthoptera of Kentucky are deserving of a word in this connection. The species are all wingless crickets with greatly enlarged hind limbs for leaping, and excessively lengthened antennæ. All have eyes of the usual size, and without exception live by preference near the cave mouths. The species most completely adapted to life in the caves is the cave cricket (*Hadenœcus subterraneus*). It is a large brown creature, so fragile that it is almost impossible to get perfect specimens. Specimens taken alive from the caves in summer, invariably died, probably because of the sudden change of temperature. I am disposed to think they could be removed in cool weather without difficulty. I have never seen this species anywhere but in caves. It occurs in all our larger caverns, however. A second species (*Ceuthophilus stygius*) resembles the preceding in general form, but has the legs and antennæ less lengthened, and is spotted with black. It is closely allied, both in structure and color, with species occurring out of doors under rocks. It is more closely confined to the region near the entrance of caves than is *Hadenœcus subterraneus*, but appears not to leave the caves. These two are the only cave crickets I have seen in Kentucky, but Dr. A. S. Packard, of Brown University, has obtained a third, which he says is associated in caves with the preceding. I have a number of specimens that agree perfectly with his description of this cricket, but they were found in every case under rocks or logs out of doors."

Coleoptera of Lower California.—At a recent meeting of the Cambridge Entomological Club, Dr. G. H. Horn discussed this subject.[5] He remarked "that about 800 species were now known to him from the region which may be divided into four faunal provinces: (1) The San Diego fauna extends down the larger part of the west coast. (2) The fauna of the highlands (so far as collected, *i. e.*, north of the middle of the State) seems to be related to that of the Central California Valley. (3) The fauna of the east coast extends through Arizona northward, and eastward down the Rio Grande. (4) The fauna of the extreme southern end of the peninsula is truly tropical in character."

New Fossil Beetles.—Mr. S. H. Scudder calls attention[6] to a

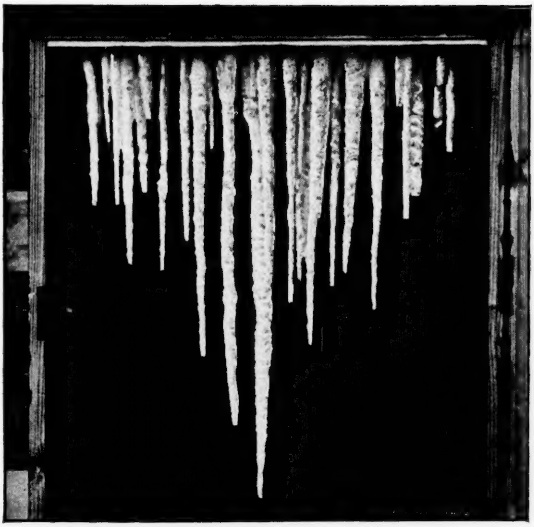
[5] *Psyche*, Nov., 1894.
[6] *Psyche*, Nov., 1894.

new family of fossil beetles established by Schlechtendahl in a recent paper on the fossil insects of Rott on the Rhine (Abh. Naturf. Ges. Halle, XX). It is named Paleogyrinidæ, and the type shows a combination of the characters of Gyrinidæ and Dytiscidæ. "Extinct types of insects of as high a grade as families are extremely rare in the tertiaries."

Reversal of Position in Insect Embryos.—Dr. G. A. Chapman summarizes[7] his own and others' observations on the phenomena associated with the change of position that occurs in the young lepidopterous larvæ within the shell before hatching. "In all cases the larva first appears on the surface of the yelk-mass as a flat plate, of which the central line is the middle of the ventral surface, and the margins are the two sides of the dorsum, still far apart. These margins, however, rapidly curl in and, at the head and tail, the young embryo soon has the cylindrical form we associate with the larva, but centrally, there remains a wide opening through which the mass of the yelk is continuous with that portion of it contained in a central cavity of the larva; this central cavity is the future alimentary canal, not yet provided, however, with any opening towards either the head or the tail. The communication between the intestinal cavity and the yelk sac gradually becomes smaller, and portions of yelk leave the sac and pass into the intestine, and contribute to the growth of the embryo. During this period, it is easy, in flat eggs like those of the Pyralides, Tortrices, Limacodes, etc., to see the embryo curled around a greater or less portion of the yelk sac, with its ventral surface towards the margin of the egg, and its dorsal surface (aspect rather than surface, as the surface is still broken by the umbilical opening) applied to the yelk sac. There is a little variation in the degree to which the yelk disappears before the umbilical opening closes, but when this takes place the larva forms a horseshoe or circle, with the venter towards the shell wall and its anterior and posterior extremities in contact. At this period, also, there are a varying number of globules of yelk free in the egg cavity around the larva; whether these are set free by the movement of the larva that now takes place, or still later by the jaw action of the larva, I am not sure, but after the movement has taken place the young larva swallows these; this swallowing of the remaining yelk may indeed be regarded as a first step towards eating its way out of the egg. Before the closing of the umbilical opening, the embryo may be regarded as an appendage to the yelk sac, attached thereto by its

[7] Entomologist's Record, Oct. 15, 1894.

PLATE XXXIV.



From photograph of Stalactite 60 centimeters long and 20 years old; formed between the years 1873 and 1893 on the ceiling of a reservoir roof arch at Bayreuth, Bavaria. Scale $\frac{1}{7 \cdot 73}$

dorsal aspect. As soon as the opening closes, however, the young larva is truly a young larva, possessing no organic connection with the other egg structures. The first use it makes of its liberty is to bend the tail forwards and, as it were, creep up its own ventral surface, assuming in this process an S or pot-hook shape, until at length its position is reversed, the dorsum being now along the circumference of the egg and the venter being central. The head and tail sometimes merely meet in the (flattest eggs), sometimes slightly overlap, whilst, in the dome-shaped eggs the head so overlaps as to take very often a central position in the vertex of the egg, forming a dark spot there, as in Acronycta, Skippers, and many others.

"The essential importance of this observation is, that it shows that the embryonic position of the nervous system is the same in insects as in vertebrates, and since it must, therefore, be identified also in the mature animal, it follows that the venter of insects corresponds anatomically with the dorsum of vertebrates, and *vice versa*.

"As regards the actual change of position itself, and the position afterwards taken by the larva, it seems to me that the important point is that the larva whilst still truly an embryo, that is, whilst still attached to the yelk and egg structures, has the venter outwards, and the dorsum towards the center of the yelk or egg; but when it becomes free it is no longer an embryo, it moves how it likes, and through the position it takes up seems to be very uniform throughout each species and and even throughout whole families; still this has little, if any, embryological significance. I have frequently seen larvæ making this S movement, and though I have called it 'creeping up its own ventral surface,' it goes on slowly, without any apparent voluntary or even movements, and appears to be due to the mere force of the growth and development of the larva. Sometimes it seems as if the lengthening of the larva led to the extremity of the tail impinging against the side of the egg-shell and instead of sliding onwards, being caught and bent up. It is associated no doubt with the completion of the growth of the dorsal surface previously defective by the large umbilical opening, and now more abundant in proportion to the ventral surface. It proceeds slowly and steadily, so that usually some progress may be noted in five or ten minutes.

"Very shortly after, what appear to be voluntary movements of swallowing take place, the remainder of the yelk disappears, and the remaining fluid is either absorbed by the larva through the skin, or evaporates through the shell; the tracheæ become visible by getting filled with air, and the larva begins the process of eating through the shell."

Cecindelid Larvæ.—H. F. Wickham describes[8] the larva of Cecindela as "a somewhat elongate, whitish grub, with a broad, metallic colored head and prothorax, and a large hump, bearing two hooks, on the fifth abdominal segment. They excavate holes in sunny spots and lie in wait for prey, with the head closing up the mouth of the burrow; when an insect comes within reach, it is seized by the long jaws of the larva and the juices extracted. I am now rearing larvæ of *C. limbalis* Klug, which I dug from holes in a clay bank on the fifteenth of April. They are easily kept in little tin boxes with damp earth, and feed readily on soft-bodied larvæ of wood-borers. The pupa is figured by Letzner and is represented as bearing on the fifth abdominal dorsal, two spines corresponding to the hooks on the same segment in the larva."

Social Economy of the Hive Bee.—In a recent presidential address before the Biological Society of Washington, Dr. C. V. Riley described the social organization of the hive bee.[9] "Each bee," he said, "labors for the good of the commonwealth of which it is a member. Of them it might well be said:

Salus rei publicæ lex.

It is the welfare of the colony which directs the actions of all, and not the will of the queen. Indeed, it would seem that the latter performs her important function—that of supplying the hive with eggs—only when the workers will it, their own condition of prosperity as regards stores, or their anticipations of the future needs of the colony as regards population, causing them to supply the queen liberally with food rich in nitrogen—a partially digested substance, or a gland product, or perhaps, a mixture of both, which she alone cannot produce, yet without which any considerable production of eggs is an impossibility. As Evans remarks:

'The prescient female rears her tender brood
In strict proportion to the hoarded food.'

"We must, then, credit the industrious and provident workers with the chief influence in shaping the policy of the hive. They are the *servum pecus*—the living force—of the colony. And to the end that order and efficiency of effort may prevail, they have, we find, a marked division of labor. In the normal condition of the hive the young workers care for the brood—a labor which they take upon themselves

[8] Can. Entomologist, June, 1894.
[9] Insect Life, September, 1894.

within two or three days after issuing from the cell. The glands which secrete a part of the food required by the developing larvæ are active during the earlier part of the life of the worker. Later, these nurses become incapable of doing their work well as the gland system becomes atrophied. When a few days old they take short flights, if the weather favors, but seldom commence gathering stores before they are fifteen days old. Wax production is more essentially a function of the workers in middle life, and it is particularly noticeable that those bees fashioning the wax into combs are principally of this class. Many of those acting as foragers do, however, secrete wax scales, which are doubtless, in the main, utilized. Among the outside workers and hive defenders some bring honey only on certain trips or for a time, others honey and pollen, others water, and yet others propolis or bee glue to stop up crevices and glue things fast. Meanwhile, some are buzzing their wings at the entrance to ventilate the hive, and others are removing dead bees, dust or loose fibers of wood from the inside of the hive or from near the entrance, or are guarding this last against intruders, or perhaps driving out the drones when these are no longer needed."

Notes on New Hampshire Lepidoptera.—Mr. James H. Johnson, Pittsfield, N. H., in a letter to the editor of this department, recently, included the following notes on Lepidoptera in his region: "I have one specimen of *Colias interior* from Charlestown. This, I notice, Maynard calls 'accidental at Waterville, Me.' One specimen of *Debis portlandia* I took at Webster, one *Limenitis arthemis* (*proserpina*) at South Sutton, one *Thanaos brizo* and several of *Neonympha eurytris* at Charlestown. I have a pair of the *Chionibas jutta* from Orono, Me.

"Of the moths, I have one each of *Catocala relicta* and *C. relicta* (*bianca*) one pair of *Eacles imperialis*. These three were taken at South Sutton, Va. I find *Eucronia maia* is quite common in one place here at Pittsfield. Have not noticed it elsewhere. I see Dr. Harris called it rare in Mass."

Hemiptera of Buffalo.—One of the most valuable of recent faunal lists has just appeared in the Bulletin of the Buffalo Society of Natural Sciences (Vol. V, No. 4). It is "A List of the Hemiptera of Buffalo and Vicinity," by Edward P. Van Duzee. It "enumerates all the described Hemiptera to and including the Jassoidea known to inhabit the vicinity of Buffalo, N. Y. The limit of 70 miles, adopted by

Mr. David F. Day in his Catalogue of the Plants of Buffalo and Vicinity, has been followed by the author * * * but nearly all the species have been captured within a radius of 20 miles of this city." The Psyllidæ, Aphididæ and Coccidæ have not been included in the list which enumerates 378 species, and mentions 25 undescribed species that have been found.

In the same Bulletin Mr. Van Duzee publishes Descriptions of some New North American Hemipterous Insects, belonging to the following genera: Idiocerus, Platymetopius, Allygus, Deltocephalus, Athysanus, Entettix, Scaphoideus, Thamnotettix, and the new genera here characterized, Tinobregmus and Xestocephalus.

ARCHEOLOGY AND ETHNOLOGY.[1]

The Age of Certain Stalactites.[2]—The fact has been recognized for some time among scientists that the formation of stalactites, under favorable circumstances may take place in a relatively short time.

Nevertheless, observations upon the exact period required for the growth of given examples have been rather rare, for while there has been abundant opportunity to compute the age of stalactites at railway bridges and tunnels, the various dangers which beset these delicate growths in such places have generally put a considerable limit to their age, and deprived them of conspicuous size. It may, therefore, be of interest to state an instance where not only the time of growth but also the exact size of a stalactite can be given with absolute precision.

In the year 1873 the city of Bayreuth (Bavaria) built a reservoir for drinking water three kilometers southwest of the town. This so-called Lasser Reservoir is built in a *Keupersand* soil which contains exceedingly slight traces of lime. The water used in the basin comes out of the *Keuper*, (the uppermost of the three subdivisions of the Triassic period), and likewise contains lime, though in very small quantity. At the point shown in the illustration a spot on the ceiling of the arch (built across the tank to protect the water from pollution H. C. M.) stalactites of remarkable size had formed in 1893. Supposing that they had begun to form by the infiltration of surface water through the arch immediately upon its completion in 1873, they could not have been more than twenty years (1873–1893) old, and as the photograph recently taken from nature shows their length to be between 60 and 80 centimeters, they must have grown on an average of from 3 to 4 centimeters a year. The reservoir was first used in 1874, the tank under the arch remaining full of water until the present year, when in the course of the summer, the water was drawn off for repairs, and an opportunity afforded of observing and detaching some of the stalactites. A great number of the finest specimens were broken through the ignorance of workmen. In a damp walled chamber adjacent to, though not included in the area of, the basin, hung whole rows of stalactites from 20 to 30 centimeters long. These were extremely fragile and very difficult to remove without breaking.

[1] This department is edited by H. C. Mercer, University of Pennsylvania.

[2] Translation from the original German.

A careful examination of the structure of the reservoir building showed that the stalactites must have formed as follows:

The reservoir's arched roof from which they hung was built of bricks laid in cement (probably the kind known in America as German Portland, H. C. M.). Slight fissures had formed in the cement through which the water of the surface (rain water H. C. M.) had trickled. This down trickling water had dissolved portions of the cement, and then evaporating, had first caused a formation composed of particles of lime dissolved from the cement. This formation was the starting point of the stalactites. On it had been precipitated very fine particles of the reservoir water, leaving after they had evaporated a further residuum of lime upon the already existent pendant.

This view is strengthened by the fact, that since the building of another more recent (so-called Fuchstein) reservoir 3 kilometers west of Bayreuth, stalactites 2.5 centimeters long have shown themselves hanging in the same way from the cement ceiling of the roofing arch. Moreover, if indentations are scratched in the cement, pendent accumulations of lime are soon formed, which, however, are not hollow in the middle like the stalactites.

Finally, as the result of an experiment, the following method for producing stalactites artificially, may be mentioned:

Take a common hectoliter cask. Make a hole in its bottom. Plug this hole with a wooden plug so wound with tow that the water may trickle through it in very small quantities. Around the end of the plug on the outside of the bottom of the cask, spread cement (German Portland cement, H. C. M.) in which a slight fissure should be left. Then fill the cask with the water containing lime in solution and place it in the open air. Hang a piece of tow on the fissure in the cement so that the water trickles upon it, and stalactites will form very rapidly. In this way I made a stalactite 5 centimeters long inside of 8 weeks.

FRANZ ADAMI.

Bayreuth, September 30, 1894.

Note by the Editor.—A very hard crust of stalagmite, covering a loam bed with rhinoceros teeth and human relics, overlaid the cave floor of Kents Hole (near Torquay, England) in which Mr. McEnery says (1825) that he found in no instance breaches or openings, "but one continuous plate of stalagmite diffused uniformly over the loam." Schmerling who (in 1832) used to climb down into Engis Cave (near Liege, Belgium) by a rope tied to a tree, and after a long crawl, stand in the mud to superintend by torchlight, workmen digging in a wet

hole, had to break through a stalagmitic floor hard as marble and cut five feet into a breccia nearly as hard, to find the famous skull now in the University of Liege.

But the presence of these crusts, though serving satisfactorily to separate diverse accumulations on cave floors one from another, is no longer regarded in Europe as evidence of the great age of relics so entombed.

In the Wyandot Cave (right bank of Blue River, 5 miles from its mouth in the Ohio, Crawford Co., Indiana) a hole has been artificially battered in the side of one of the innermost large stalactites called "The Pillar of the Constitution,"and it appeared from the observations of Professor Collet (Ind. Geolog. Survey, 1876–77–78, p. 467) and Mr. Hovey who found (as I did in June, 1894) granite pebble hammerstones lying in a mass of splinters near the hole, and Mr. H. W. Rothrock, who (in 1877–78) found besides hammerstones, a deer horn "pick" or prying tool, close by, that Indians had battered out the hole with the stone hammers to get fragments of carbonate of lime for some purpose (possibly trinket making) not yet determined.

A crust of stalactite 10 inches thick has since crept over the bruised edge of this unique quarry, and Mr. Hovey thought (Celebrated American Caverns, p. 139) that "at the known rate of increase, it must have required 1000 years for the wrapping to attain its present thickness of 10 inches, and that length of time has, therefore, elapsed since this 'alabaster' quarry was worked."

Professor Adami's above statement which omits, however, a chemical analysis of the cement referred to, is one of the sort of valuable observations which has shaken faith in the worth of all age tests based on stalagmite or stalactite. If for a thousand years the still standing forests have helped dampen the roof of Wyandot Cave, if rain has kept falling at an equal rate all that while, and if water always equally charged with lime has gone on trickling through the ceilings ever since, then what happened in twenty years to rain water and cement at Bayreuth might have taken fifty or a hundred times as long to happen to rain water and limestone in Indiana. But we can hardly imagine a case where in a cave care enough would have been taken, and time enough spent in measuring the yearly increment, or still more where the inferred conditions of uniformity reaching back into a little known geological past, could have been weighed.

H. C. MERCER.

Indians Mining Lead.—Mr. Benjamin Pursell, of Kintnersville, Bucks County, Pa., told me in September, 1891, as a well known story in the Delaware Valley, that Indians in the last century had shown members of the Ridge family, then living on Ridge's Island, lead ore in situ, at a spot never since discovered in the neighboring hills.

More definite still is the lead story of New Galena, Bucks County, Pa., at third hand. Somewhere in the middle of the century Elijah and Abraham Campbell, of Plumstead, told John M. Proctor, now of Blooming Glen, who wrote me in December, 1891, that straggling Indians coming to hunt along the north branch of the Neshaminy, between 1790 and 1808, had often taken them as boys to a place near the mouth of the "Hartyhickon" (now the property of Mr. Arthur Chapman). There they disappeared in the woods to return with their arms full of lead, with which they made bullets.

I took these for local tales till I was surprised to hear J. M. Kessler, at Hummel's Wharf, Snyder County, Pa., tell me the same story, while pointing to the hills across the Susquehanna as its scene. But I came nearest of all to the legend when Reuben Anders, of Little Wapwalopen, Luzerne County, Pa., gave me it first hand. He had seen the Indian who had spent the night with his grandfather and offered to show him a mineral wonder on a hill called Councilkopf. Though the latter was afraid to follow the red man alone, one Harman had gone hunting with two others, who when bullets had given out had gone into the woods and returned with loads of lead. If untrue, it is hard to see why this lead story has so seized the popular mind. But when we realize, as I am informed, that lead rarely, if ever, occurs pure in nature, but as galena, which, if mixed with lumps of limestone, requires about 1200 degrees (Centigrade) of heat to smelt by drying out the carbonic acid and removing the sulphur, it is to be doubted whether, given the galena, any such offhand bullet-making in the woods could ever have taken place.[3]

Squier and Davis found galena ornaments in ancient Ohio tumuli. Mr. Clarence B. Moore showed me a lump excavated by him from a St. John's River (Florida) mound, and modern Sioux ornament their catlinite pipes with lead, but no digging has yet proved that mound

[3] Some specimens of galena, recently obtained through Mr. Alfred Paschall, from the prospective mine now working in the bed of the North Branch of the Neshaminy, on the farm of Henry Funk (New Britain Township, Bucks County, Pa.), would not melt in a red-hot crucible, but splintered into fine fragments, as did other fragments when held directly in the bellows fire.

builder or Indian in pre-Columbian times regarded galena as other than a hard, glittering stone to be pounded or rubbed into trinkets.[1]

Still we know that the Rhode Island Indians very soon learned the art of pewter casting from Roger Williams' colonists, and the question therefore, is, had Indians in Eastern Pennsylvania by 1780–90 learned from white men how to smelt bullets from galena for their newly acquired guns?

Whether or not these lead tales furnish us with an archeological clue of importance, they seem less strange than the story told me on July 12, 1893, by Charles Keller (now 84 years old), of Point Pleasant, Bucks County, Pa., as related to him sixty years ago by his father, Christopher Keller. About the end of the last century Peter Keller, Christopher's brother, had refused to do some iron work for a band of Indians at his blacksmith shop, on Tohickon Creek, above Stover's mill (the present Redding Meyers farm,) about six miles above its mouth on the Delaware River. When he pleaded as an excuse that his supply of charcoal was exhausted, the Indians went into the forest and after nearly a day's absence returned with a basket full of "stone" (anthracite) coal, with which he did the job.

H. C. MERCER.

[1]After the present pages were written, Mr. Walter Chase, of Madison, Wisconsin, showed me a small figure of a turtle of cast lead found by him at a surface Indian camp site in 1889 on the shore of Lake Wingra, two miles southwest of Madison. Dr. Hall, of Madison, had another plowed up by a farmer in 1891, with a stone axe and four or five arrowheads, from an effigy mound shaped, itself, somewhat like a turtle, on the shore of Lake Mendota, near Madison. Two perforated discs of cast lead have also been found by farmers in Dare County, Wisconsin, and are now in the possession of neighboring collectors. Galena occurs in Southern Wisconsin in small, loose masses in a very pure state.

PROCEEDINGS OF SCIENTIFIC SOCIETIES.

Boston Society of Natural History, November 7.—The following paper was read: Professor George Lincoln Goodale, An account of the Ware Collection of Blaschka Glass models of Flowers in the Harvard University Museum. With illustrations.

November 21st.—The following paper was read: Dr. George A. Dorsey, "The Peruvians, prehistoric and modern." Stereopticon views were shown.

Samuel Henshaw, *Secretary.*

New York Academy of Sciences, Biological Section, October 22.—The following papers were read: Professor N. L. Britton, and T. H. Kearney, Jr., "On a Collection of Texano-Mexican Plants,"—new species and altitudinal notes; Professor E. B. Wilson, "The fertilization and polarity of the egg in *Toxopneustes lividus.*" The study of extensive series of sections fixed by sublimate-acetic and stained by Heidenhain's iron-hæmatoxylin fails to give any evidence of a "quadrille of the centrosomes." The archoplasm is wholly derived from, or formed under the influence of a substance derived from the spermatozoön and situated not at the apex but in or near the middle-piece. Regarding polarity, the continuous observation of a large series of living eggs shows that the definitive egg-axis has no constant relation to that passing through the excentric egg-nucleus but may form any angle with it. The first cleavage passes approximately through the point of entrance of the spermatozoon as described by Roux in the frog. Dr. Bashford Dean, "On the breeding habits of Lepidosteus from observations at Black Lake, N. Y., May, 1894;" Professor H. F. Osborn, "On the Proceedings of the Biological Section of the British Association."

November 12.—N. L. Britton, "Problems in Plant Evolution," noting from the side of Paleobotany the centralized position of Algæ and the probable affinities of pteridophytes and bryophytes. G. N. Calkins, "A little known phenomenon in the life history of *Stentor coeruleus.*" The free swimming Lieberkuhnina of Bütschli was shown to be (as Claparède and Lachman had earlier believed) an embryo *Stentor.* H. G. Dyar, "A classification of Lepidopterous larvæ according to setiferous tubules," giving data for the establishment of six

super-families. S. F. Clark, "The breeding habits of Alligator." H. F. Osborn, "The skull structure of Titanotheres."

BASHFORD DEAN, *Rec. Sec.*

National Academy of Sciences.—The following papers were read at the meeting in New Haven, Oct. 30., Nov. 1.—An indirect experimental Determination of the Energy of Obscure Heat, William A. Rogers; Determination of the Errors of the Circles of an electrotype copy of Tycho Brahe's Altitude Azimuth Instrument now in possession of the Smithsonian Institution, William A. Rogers; The Winnebago County, Iowa, Meteorites and the Meteor, Hubert A. Newton; Literal Expression for the Motion of the Moon's Perigee, George W. Hill; Atmospheric Dust and Aqueous Precipitation in Arctic Regions, William H. Brewer; Further Researches on the Polar Motion, Seth C. Chandler; The Relation of Gravity to Continental Elevation, Thomas C. Mendenhall; The Legal Units of Electrical Measure, Thomas C. Mendenhall; On derived Equations in Optics, Charles S. Hastings; On a method of eliminating Secondary Dispersion, using ordinary silicate Glasses only, Charles S. Hastings; The Chemical Nature of Diastase, Thomas B. Osborne, (Introduced by S. W. Johnson); Some Features in the Development of Brachiopods, Charles E. Beecher, (Introduced by O. C. Marsh); On the Presence of Devonian Fossils in Strata of Carboniferous Age, Henry S. Williams, (Introduced by O. C. Marsh); On the influence of Insolation upon Culture Media, and of Desiccation upon the Vitality of the Bacillus of Typhoid, of the Colon Bacillus, and of the Staphylococcus aureus, John S. Billings; Report on Photographing Meteors, William L. Elkin, (Introduced by H. A. Newton); Biographical Memoir of F. V. Hayden, Charles A. White; Geographical and Bathymetrical Distribution of the Deep Sea Echinoderms, discovered off the American Coast, north of Cape Hatteras, A. E. Verrill; On the effect of Pressure in broadening Spectral Lines, A. A. Michelson; Remarks upon the progress of work upon a Handbook of the Brachiopoda, James Hall; Note upon the Occurrence and Distribution of the Dictyospongidæ in the Devonian and Carboniferous Formations, James Hall; Infra red Spectrum, S. P. Langley; On a certain Theorem in Theoretical Mechanics, J. W. Gibbs.

The Biological Society of Washington, December 1st.—The following communications were read: Mr. B. T. Galloway, "The Physiological Significance of the Transpiration of Plants." Mr. F.

H. Knowlton, "The Amount of Water Transpired by Plants," Prof. B. W. Evermann, "The Redfish of the Idaho Lakes." Mr. Charles T. Simpson, "On the Validity of the Genus Margaritana."

FREDERIC A. LUCAS, *Secretary.*